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ATTENTION AND DISTRACTION.

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CHAPTER I.

Introductory.

On Certain Physiological Concepts Involved in Theories of Attention.

In our later examination of various theories of attention, we shall find certain physiological concepts introduced. The validity of their use in the maintenance of different positions will be considered in connection with the discussion of the theories in whose favor they are adduced. But the concepts themselves are neither so generally accepted nor so well established by physiological experiments as to admit of our introducing them without a preliminary definition of their significance and value in physiology itself. The two topics requiring brief notices of this sort are *cerebral localization* on the one hand, and *Bahnung* and *Hemmung*—neural reinforcement or facilitation and inhibition — on the other hand.

Cerebral Localization. The physiological controversies over localization in the cortex, carried on by Hitzig, Munk, Ferrier, Goltz, Flourens, Horsley and many others, have resulted in the establishment of a few definite principles of cerebral functioning. These are summed up by Wundt as the principles of the Indifference of Function; the Substitution of Function; and the Localization of Function.[1] The same three principles are stated less formally by Waller in

[1] '*Physiologische Psychologie*,'' IV ed., Vol. I, pp. 235, 236.

the following words: "The clinical history of a dog or of a monkey having suffered a removal of some portion of the Rolandic area altogether negatives a strict localization of function, and at most suggests its local concentration. . . We thus picture the cortical organ in a semi-fluid state of differentiation, still variable by new instruction, rather than as a petrified and invariable collection of specialist organs tied down to particular functions and exclusively performing these functions."[1] In the same article Waller shows very clearly that the differences in theories of localization have been mainly due to a widely differing and even contradictory use of words for phenomena that were identical, and to the inferences drawn from these words.[2]

Neural Reinforcement (Facilitation) and Inhibition: "Bahnung" and "Hemmung."

We do not need to take any special notice of the physiological character and basis of inhibition. While there is still discussion in regard to its mechanism, the fact of inhibition, as tested, for instance, in the action of the *vagus* nerve on the heart, is well known and has frequently been demonstrated.

It is only mentioned here in connection with the opposite and more hypothetical process of neural reinforcement or facilitation. This latter idea plays a most prominent part in the theories of attention put forth by Exner and von Kries. They both refer to it as "a physiologically well established fact." I have searched the recent physiological journals and archives for confirmation of this statement, and have been unable to find mention of it. Exner, however, in his "*Entwurf zu einer physiologischen Erklärung der psychischen Erscheinungen*" gives his own physiological proof of the existence of *Bahnung*, and I quote rather fully from his report of the observations on which he bases this concept.

Exner uses *Bahnung* to mean just what we expect the word to mean—"the opening of a path."[3] It is a *facilitation* of the course of the nerve current, due either to its reinforcement by charges from the centre, or to the lowering of the limen of discharge.[4] Exner states that he first reached his conclusion that there must be *Bahnung* in connection with his experiments on reaction time. But he maintains also

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[1] "On the Functional Attributes of the Cerebral Cortex," *Brain*, Parts LIX and LX, 1892.

[2] *Op. cit.*, p. 346 *seq.*

[3] "*Entwurf zu einer physiologischen Erklärung der psychischen Erscheinungen*" (1894), p. 76.

[4] *Op. cit.*, p. 82.

that he has succeeded in proving its existence experimentally by the following experiments with animals.[1]

(1) Pass an electric shock through the paw of a rabbit. Since the reflex contraction follows in an animal whose brain has been severed from the spinal cord, this reflex must be controlled by a cord centre. Now lessen the electric shock until it is too weak to produce any contraction. Apply this feeble shock to the paw, and apply to the cortex a shock also too feeble to produce any result alone. If the two are given in rapid succession, one reinforces the other, and the paw is flexed. Exner states that this is not a "summation," since in summation the reinforcing stimuli follow the same path as the reinforced. Summation, however, is one particular form of *Bahnung*.

(2) A passing reference is made to the fact that the reflex contraction following a needle prick is much stronger if we give close attention to the prick. Assuming that the cortex is in a state of excitation in attention, Exner uses the phenomenon as an instance of cortical *Bahnung* of a spinal cord centre.

(3) Sternberg has observed the *Bahnung* of other reflexes in the course of his studies on *Hemmung, Ermüdung, und Bahnung der Sehnenreflexe im Rückenmarke.*[2]

(4) *Bahnung* of the other sort, where the limen for discharge is lowered, is present in the action of the *vagus* nuclei. They lie in the *medulla oblongata*, and are connected by commissural fibres. If a section is made through these fibres, each half of the body breathes independently. If the two lungs move at the same time when the nuclei are connected, this must be because an increasing charge in either affects the other, so that it is more ready to discharge; and the actual discharge of the two must be simultaneous.

(5) The fifth case is taken from von Grossman's investigations[3] of the three nuclei concerned in the breathing of the rabbit, one controlling the facial muscles, one the *vagus* muscles, and one the thoracic muscles. If one of the three is separated from its connection with the other two, these will continue to do their work; but the limen for their release of the inspiratory impulse is raised, and the animal makes movements which we call "gasping for breath." So that in this case we find the stimulation of one aiding the function of each of the others, and all three together reaching their exci-

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[1] *Op. cit.*, pp. 76-82.

[2] *Wiener Akadem. Sitzber., Bd.* C, *Abth.* III, *Juni*, 1891.

[3] "*Ueber die Athembewegungen des Kehlkopfes*," I *Theil. Wiener Akadem. Sitzber., Bd.* XCVIII, *Abth.* III, *Juli*, 1889.

tation limen at a lower point than is possible for any two, or for one alone. Notice that here we plainly have *Bahnung*, not as a reinforcement of a current, but as an influence which allows currents to pass off, or to discharge, more easily; *i. e.*, as a facilitation only.

Other physiological concepts will frequently recur in the subsequent chapters; but they are either so generally recognized as to need no special notice, or so intimately associated with the psychological theories we shall discuss later that we may advantageously consider them in their psychological connection rather than in this introductory chapter.

CHAPTER II.

The Descriptive Theories of Attention.

Before attempting to give any account of our experimental investigation of the effect upon sensible discrimination of "free" and "distracted" attention, we shall pass in review some of the most important and typical theories of attention. The whole subject is still in a somewhat chaotic state. A summary of certain representative theories will tend to bring order out of the chaos and provide a basis for a more definite and intelligible treatment of isolated experiments. The old phrase, "*Quot homines tot sententiæ*," may well be taken to represent the number and variety of theories of attention advanced by the psychologists of this century. To indicate the importance of the problem and the incompleteness of its solution, I may quote from one of the most recent investigators of the subject, Dr. Heinrich of Zurich.

> In the series of questions which concern psychology, attention takes the first and most important place. It is regarded as the fundamental condition of every human activity. In scientific investigation and thought, in practical action, in learning and teaching, attention is always a prerequisite if anything is to be accomplished. No wonder, then, that every psychology seeks to answer the question how those phenomena arise to which we give the name of Attention. * * * Yet even now psychology has no theory, only many theories. Even now, as in the "good old times," each psychologist seeks to develop a theory of his own; and very few of these can be characterized as scientific.[1]

Yet in the general confusion two tendencies are discernible, according to which the theories may be roughly divided into two classes. One class includes the more purely "descriptive" theories, with more emphasis on the physiological aspect of attention, and less distinction between psychological

[1] "*Die Aufmerksamkeit und die Funktion der Sinnesorgane*," Ebbinghaus' *Zeitschr.*, IX, 5 and 6, Jan., 1896, pp. 342, 343.

and physiological, primary and secondary phenomena. The other class includes the "explanatory" theories, with stronger emphasis on the psychological aspect of attention. Naturally, these two principles of classification intersect, and no theory is merely descriptive or explanatory, physiological or psychological. But the distinction is a matter of convenience in the present discussion, and it also expresses a real difference in the general tendency of the theorists that largely accounts for the great dissimilarity in their treatment of the question.

The present chapter takes up the more exclusively descriptive and physiological theories of attention, recognizing as its three important types the sensory, motor, and sensori-motor theories. Any ambiguity in the meaning of these terms will be cleared away as we take up in detail the several types.

I. *Theories of attention as motor.* We turn first to the motor theory, a theory which holds that attention is primarily a term covering a group or series of motor changes; processes preceding or following these changes are not an essential part of attention itself.

As the most complete presentation of a motor theory, Ribot's volume on "The Psychology of Attention" demands careful consideration. This work, however, is so well known that we need attempt no review of the discussion as a whole. We shall merely refer briefly to passages that state Ribot's position on certain contested points. After a detailed description of the "motor mechanism" and "muscular accompaniments" of attention, he definitely affirms that "these motor manifestations, with the state of consciousness which is their subjective side, *are* attention."[1] They are "its constitutive elements." And again, in speaking of voluntary attention, and of the nature of will, he maintains that "our only positive conception of will" is our idea of the action of the voluntary muscles.[2] He recognizes the intimate association between the power of attention and the frontal lobes[3]; but the correlation offers no obstacle to his theory, since he also regards the frontal lobes as the physiological organ for the regulation of the motor centres. He calls attention to three large groups of muscular concomitants[4]: (1) Vaso-motor phenomena, including both peripheral and central hyperæmia; (2) Respiratory movements; (3) "Expressive" movements of other muscles of the body. Under

[1] "*Psychologie de l'Attention*" (1889), p. 38.
[2] *Op. cit.*, p. 73.
[3] P. 67.
[4] *Op. cit.*, p. 20.

the last rubric he gives the often quoted descriptions of the muscular mechanism of attention directed outwards toward external objects, and of attention directed inwards toward internal objects.[1] Ribot finds his greatest difficulty in the application of the motor theory when he enters upon the consideration of attention to abstract ideas and concepts. He admits that the motor elements here are very feeble, but adds that "this is in accordance with the experience that abstract reflection is impossible for many, difficult and fatiguing for almost everyone."[2] He finally explains the difficulty by maintaining that abstract thought deals only with words. A motor element is found in our word ideas, though its form varies somewhat according as the individual is more or less of the motor, visual or auditory type.[3]

It seems unnecessary to give any further summary of a theory so generally known as Ribot's.[4] Before we review any other theory, however, there are three criticisms to be passed on Ribot's discussion of the attention as essentially motor. (1) He assumes that the alternative to his own theory is a denial that there is any essential connection between muscular manifestations and attention. "The *crucial* test would consist in seeing whether a man deprived of all power of external and internal motion, and of that alone, would still be capable of attention. The case is one that can not be realized."[5] But the motor phenomena may be regarded as the inseparable concomitants of attention and yet not as its "constitutive elements." They may be only one class among several of the conditions of attention. This is Külpe's view of their significance.[6] Or they may be fundamentally related by the constitution of the nervous system to other processes that have a better claim to the name of attention. This accords with Bastian's view of attention as essentially sensory, yet inseparably bound up with motor activity.[7] No theory denies that the muscles have an important part to play in the process of attention. (2) Ribot does not give any materials for the resolution of a difficulty that presents itself after a moment's reflection upon his explanation of at-

[1] Pp. 27, 28.
[2] *Op. cit.*, p. 86.
[3] *E. g.*, p. 84.
[4] Ribot's treatment of the dependence of attention on affective states, and his definition of attention as an "intellectual mono-ideism," need not concern us here. Whatever may be the logical outcome of either position, neither is so developed by Ribot himself as to modify his conception of attention as essentially motor.
[5] *Op. cit.*, p. 88.
[6] "Outlines of Psychology," tr., p. 437.
[7] *Brain*, 1892, pp. 10, 11.

tention to abstract ideas. He states that the only motor element present in such attention is in the word itself, but the character of this motor element varies in individuals according to their mental type, whether visual, auditory, or motor.[1] Now if the motor changes constitute the very nature of attention, attention itself must be dissimilar in persons of different types, and those in whom the motor element is most distinct will have the greatest power of attention. Different degrees of attention would be merely different degrees of motor adjustment. (3) Ribot fails to analyze the idea of movement. Ordinarily he refers to the purely physiological aspects of movement, and speaks of attention as their sum. But even when he is definitely concerned with the psychological aspect of movement,[2] he refers to ideas of movement without analysis or definition of the term. The phrase, "idea of movement," can properly be used only in two senses: to mean (a) the idea to which movement itself is a stimulus; and (b) the idea upon which is based the knowledge that we have moved. Of the two definitions, the first presents the more legitimate use of the term. Just as the idea of sound is that to which the excitation of the auditory organ is a stimulus, so the idea of movement is the idea to which the excitation of the articular sensibility is a stimulus. By the second definition "ideas of movement" belong to the same class as ideas of magnitude or distance, and include in their content sensations of any and all sorts. But, in either case, the content of the term is a homogeneous or a heterogeneous mass of sensations. So it has come to pass that other psychologists, studying the same phenomena, reject Ribot's statement that the attention is essentially motor, and regard it as essentially sensory. The separation of these sensory and motor theories is an artificial one, but before completing their reconciliation, the differences between the two should be presented from the point of view of the "sensory" theorists.

II. *Theory of attention as sensory.* The sensory theory has been presented in a rather fragmentary way, by writers more directly concerned with the criticism of the motor theory than with the systematic formulation of their own views. The most complete statements of the position yet made are those of Bastian[3] and Marillier.[4]

Marillier defines attention as "a state of consciousness which is the result of the temporary predominance of one

[1] *Op. cit.*, pp. 84-88.
[2] *Op. cit.*, *e. g.*, p. 72.
[3] "On the Neural Processes underlying Attention and Volition," *Brain*, LVII, 1892, pp. 1-34.
[4] "*Sur le Mécanisme de l'Attention*," *Rev. Phil.*, 1889, pp. 566-588.

representation over the representations which are coexistent with it at any given moment."[1] He maintains that Ribot's analysis of the muscular movements in attention really proves "that what is essential here is the reinforcement of a representation, and that sensations are what determine this reinforcement. Movements are only a condition, as indispensable as you choose, but not an element of the phenomenon. What he [Ribot] succeeds in showing, after a long analysis of the mechanism of the voluntary attention, is the very important part played by muscular sensations in the memory of images and words."[2] Marillier goes on to say that after admitting that motor phenomena often play an important part in attention through the sensations which they excite, we must still protest against the implication that such peripheral excitation is the only way in which a representation may be reinforced. Account must also be taken of the interaction of the centres upon one another.[3] His objection to Ribot's definition of attention as essentially motor is, then, up to this point, a two-fold objection.

(1) Admitting that motor phenomena have often a prominent part in attention, we must at the same time trace these phenomena backward to their rise in sensations, and forward to their excitation of the sensations of movement; so that on this ground alone attention should be called sensory rather than motor. (2) Ribot's account of the motor mechanism fails to recognize the interaction of sensory centres and of motor and sensory centres, and moreover—here Marillier introduces a yet more fundamental objection—the motor adjustment is often such that it fails to reinforce, or hinders the attention. This is ordinarily the case as attention grows more profound. Excitation of the sensory centres, up to a certain degree, occasions a series of muscular changes well adapted to reinforce the original sensory excitation by the sensations to which they give rise. By connate or acquired associations of certain sensations with motor adjustment, it is possible for an originally feeble sensation to be so intensified as to absorb attention. Take, for instance, the case of a dog perceiving the faint odor left in the track of a hare. The sensation has awakened strong impulses, and his attention is so absorbed that he can scarcely hear the shouts of his master.[4] But in both animals and men, if the excitation is further increased, its propagation is no longer confined to definite motor centres, so as to produce coördinated move-

[1] *Op. cit.*, p. 566.
[2] *Op. cit*, p. 572.
[3] *Op. cit.*, *e. g.*, pp. 572, 587.
[4] *Op. cit.*, p. 574.

ments of a definite sort. It is diffused and results in irregular and disordered movements.

"They are no longer adapted to any end. A man makes gestures, speaks in a loud voice, walks back and forth, stops, starts again, waves his arms in the air, takes his hair in both hands ; his mouth works in a thousand ways; sometimes his face becomes rigid, his eyes are fixed, his brows contracted, his forehead lined with wrinkles, his hands tremble, his voice is hoarse, speech is difficult, breathing is obstructed. . . . If the excitation increases still further, there is a change again : the muscles relax, ... the heart beats irregularly and slowly, the skin becomes pale and cold, ... until at last there may be an arrest of the heart and the respiratory centre, and a total loss of consciousness." [1]

Marillier adduces as a subordinate proof of the lack of thorough cöordination of the motor and sensory centres the fact that morbid states of the attention are not ordinarily associated with motor troubles. [2]

Illustrations of similar cases of attention, not covered by Ribot's theory, are presented in an article by Sully. [3] The two most detailed descriptions can best be given in his own words :

"I was walking along a narrow lane lost in thought. I came on a lamp which shot its rays through the fog. I involuntarily stood still and fixated the lamp, thinking all the while intently on my psychological problem. When this intellectual effort relaxed, and not till then, I saw and recognized the lamp. Now here was energetic muscular action, and equally energetic concentration ; but what was the relation of the two ? That this involuntary assumption of the attitude of the seer, of the fixed head, convergent eyes, etc., somehow aided the process of mental concentration, is certain. But was this muscular adjustment the *whole* of the process ? If so, I ought surely to have been mentally occupied by no abstruse problem of psychology, but with the concrete sensible object before my eyes, even though it had been far less brilliant and imposing an object than the lamp. . . . The same partial independence of the process of attention and of the motor process appears, too, in ideational attention. If I think, for example, of a circle, with the eyes closed, . . . I not only have muscular sensations which tell me that the peripheral organ by means of which I acquired the idea is engaged, but I am aware of a motor impulse to retrace the curve of the circle. . . . But now let us suppose that I am trying to visualize, not any particular form, but merely some shade of color, say, peacock blue. In this case I certainly find the motor element much less prominent. I am hardly aware, indeed, in this instance, of any ocular strain, and should say the eyes were in the easy and natural position, described by Helmholtz as the primary position, nor does attention resolve itself in this case into a renewal of the muscular action concerned in uttering the name of the color.

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[1] *Op. cit.*, p. 576.
[2] *Op. cit.*, p. 580.
[3] "The Psycho-physical Process in Attention," *Brain*, 1890, pp. 145-165.

Indeed, I find that any thought of the name distinctly disturbs the visualizing process. And yet, though the muscular element in this case is, to say the least of it, considerably reduced, the active consciousness, the attention, is as clearly present as before."[1]

Sully compares the relation between attention and motor changes with "that which obtains between an emotion and the several sensory and motor phenomena which accompany it. Fear is always accompanied by characteristic physiological changes; . . . and fear would not be fear but for these processes which contribute, in the sensations to which they give rise, characteristic features to the mental state. Yet when Prof. W. James of Harvard College not long since proposed to prove that emotion is nothing but the result of the organic changes and correlated sensations, most persons probably regarded the proposal as paradoxical."[2]

Bastian is in substantial agreement with the parts of Marillier's and Sully's views that have here been presented. But he attacks two positions maintained by Marillier and Sully, not mentioned in the digest of their articles given above, because reserved for more extended notice here. The contested positions are: (1) The correlation of consciousness with efferent nerve currents in the cortex; and (2) the existence of motor and sensory centres in the cortex. After the discussion of these topics we shall pass on to the consideration of the second great class of theories of attention—the explanatory.

I. *The correlation of consciousness with efferent nerve currents.* With regard to the first position, the correlation of consciousness with efferent nerve currents in the cortex, Sully, Marillier, Fouillée, Waller and others maintain that there is such a correlation; while Bastian, Wundt, James, Münsterberg and others assert that motor centres or efferent currents have no correlative consciousness whatever. The point is not one of ultimate importance in the controversy between the sensory and motor theories, as is evident, indeed, from the fact that writers who agree in calling attention essentially sensory differ in regard to their ideas of "motor consciousness." The opposing theories could be reconciled on the basis already suggested, *i. e.*, the reference of the word "motor" to "ideas of movement," whether initiated solely by peripheral or also by central processes. Yet the subject is one of vital importance for the theory of attention in general, and it has been given such a prominent place in the sensori-

[1] *Op. cit.*, pp. 156-157. Sully also cites Helmholtz's instance of attention to an object in the lateral regions of the field of vision. But recent experiments made by Heinrich in the laboratory at Vienna, show that there is a change in the accommodation of the eye when the attention is directed to the side of the field of vision or to mental operations. See Ebbinghaus' *Zeitschrift*, Bd. IX, January, 1896.

[2] *Op. cit.*, p. 157.

motor discussions that it may properly be discussed in this chapter. We can only quote a few typical passages from both sides of the controversy. No one ventures to assert yet that the matter is finally settled.

To present the negative side of the issue, first, we quote from Bastian, James, Wundt and Münsterberg.

Bastian writes: "In my opinion, the ganglionic elements concerned with the motor side of this activity [*i. e.*, attention] lie altogether outside the cerebral hemispheres, just as the activity of such motor mechanisms lies altogether outside the sphere of consciousness."[1] Again he refers to certain expressions "as implying that there are motor centres in the cortex and that their activity carries with it a subjective phasis; both of which positions I, in common with James, Münsterberg and others, believe to be erroneous."[2]

In a diagram in this same article, Bastian pictures a sensory centre, one of his "kinæsthetic" centres, in connection with an afferent spinal (sensory) centre and an efferent spinal (motor) centre.[3] The fibres of the pyramidal tract connecting the kinæsthetic centres and the spinal motor centres he prefers to call "internuncial" rather than efferent, and their functioning is attended by no psychical accompaniments. And so because the kinæsthetic centre is situated on the afferent side of the nervous tract, it should not be called "sensori-motor," but merely "sensory."[4]

Prof. James does not fully agree with Bastian; for he speaks of "the distinction of sensory and motor cells as having no fundamental significance,"[5] and yet he also excludes the motor zone from the sphere of consciousness.

"If the motor cells are distinct structures, they are as insentient as the motor trunks are after the posterior roots are cut. If they are not distinct structures, but are only the last sensory cells, . . . then their consciousness is that of kinæsthetic ideas and sensations merely, and consciousness accompanies the rise of activity in them rather than its discharge."[6]

Wundt, in his last edition of the *Physiologische Psychologie*, describes the "sensation of movement" as derived from four sources: pressure sensations, articular sensations, muscular (contraction) sensations, and innervation sensations. The existence of innervation sensations in the sense of centrally excited components of the idea of movement has been fully established, but Wundt defines these central components as

[1] *Op. cit.*, p. 13.
[2] *Op. cit.*, p. 27, note.
[3] *E. g.*, p. 31.
[4] *Op. cit.*, p. 32.
[5] "Principles of Psychology," II, p. 581.
[6] *Op. cit.*, II, p. 517.

"*memorial images of previous movements*, . . . which partly introduce and partly accompany every voluntary movement. Since memorial images possess qualitatively the same sensation content as the original perceptions, such central sensations of pressure and movement, under normal conditions, will completely fuse with the more intense peripheral sensations of the same kind. But they will produce an independent effect if for any reason the peripheral sensations are suppressed."[1] The term *Innervations-empfindungen* has been so widely misused and misunderstood that Wundt gives it up in his fourth edition, and uses in its place more general terms, "central components," or "central sensations." Other passages, showing Wundt's definition of the term in the third edition of the *Physiologische Psychologie*, and elsewhere, are quoted by Prof. Titchener in *Mind*, N. S., Vol. II, 1893, p. 143. Münsterberg's analysis of the sensation of innervation in *Die Willenshandlung*, pp. 75-88, reaches the same results.

On the affirmative side of this question concerning the association of consciousness with motor processes, we quote first from Marillier.

"The feeling of muscular effort, as the writings of William James and more recent experiments have proved, can be wholly traced back to muscular and tactile sensations; it disappears in anæsthesia. But beside this feeling, there is another, a feeling of cerebral effort, of the more or less easy functioning of the sensory and motor centres."[2]

Sully takes the same position in the following statement:

"If every psychical difference of quality is to have its corresponding physiological difference, there is some *a priori* reason for correlating the great and profound difference between passive sensation and active consciousness with the most important and radical difference observable in the nervous process, viz., that between the afferent and efferent current."[3]

In commenting on Sully's paper, Fouillée expresses the same opinion more strongly.

"Münsterberg denies that motor impulse is accompanied by any psychical phenomenon before movements have taken place in the limbs through muscular action. This negation is entirely gratuitous. Everything that produces rupture of cerebral equilibrium must produce a contrast in the cœnæsthesia, and contrast is the very condition of distinct consciousness. Now motor discharge, even when it is spent not upon muscles, but intra-cerebrally in the form of innervation of sensory and ideational centres, involves a sudden transformation of tension into *vis viva*, of potential energy into actual energy. This sudden rupture of physical equilibrium

[1] *Phys. Psych.* (IV*te Aufl.*), *S.* 425.
[2] *Op. cit.*, p. 586.
[3] *Op. cit.*, p. 155. *Cf.* "The Human Mind," I, p. 122.

must have its psychical counterpart; it constitutes cerebral work of a very different nature to cerebral passivity, and we call this work the substratum of attention. We are not conscious of the centrifugal current once let loose upon the muscles beyond the cerebral centres; we then experience, as a sort of *choc en retour*, mere passive muscular feelings. But we are conscious of the *starting* of a centrifugal current at the instant it is liberated in the brain."[1]

Waller, in his special study of the sense of effort, reaches the conclusion that effort and fatigue "are in major part central, in minor part muscular."[2] In the introduction to his account of his experimental study of fatigue, Waller refers to the two sides of the question, and the proper method for its solution, in the following words:

"The opinion that the sense of effort is a subjective concomitant of the outgoing nerve flow, is not in fashion with most working neurologists of the present day. Bastian goes the length of referring to it as "a doctrine now disproved," and Ferrier "is thoroughly in agreement with Bastian as to the sense of movement being dependent on centripetal impressions and not on outgoing currents." There is, however, as Dr. Jackson says, no reason why states of consciousness should not attend activities of motor as well as of sensory cells. . . . . In fact, as it seems to me, the proposition and its negative are of equal weight, equally admissible as statements of opinion, equally unproven and undisproven by the arguments invoked for and against them. *A priori* tenability is not proof, but failure of proof is not disproof. And I cannot admit as a positive premise either the opinion of Bain or the dictum of Bastian — the first to the effect that we know our movements by a central sense of motor emission, the second to the effect that the sense of effort as a concomitant of central motor innervation has been disproved. An appeal to our own feelings of effort does not prove one thing or the other; observations of clinical phenomena leave the matter unsettled; experiments on animals give answers which must be dressed out with interpretation and inference. There remains, however, in my opinion, very definite and accessible *objective* evidence to be obtained by the study of the manifestations of fatigue on man quite apart from introspective self-analysis."[3]

The "objective evidence" referred to in the paragraph just quoted was collected from a series of experiments with the sense of fatigue. The whole experimental proof proceeds upon the assumption of the similarity of "the sense of movement" and "the sense of fatigue."[4] He argues that "the incidence of fatigue" (which is the *result* of movement) may be taken as a guide to the incidence of "effort" (which is the concomitant of movement).[5] Waller concludes that effort is

[1] "Remarks on Mr. Sully's paper," *Brain*, XIII (1890), pp. 352, 353.
[2] "The Sense of Effort: an Objective Study," *Brain*, XIV (1891), p. 247.
[3] *Op. cit.*, pp. 179, 180.
[4] *E. g.*, pp. 187, 188.
[5] *Brain*, 1892, p. 380.

dependent on changes both at the centre and at the periphery. But it is not clear just what he means by this conclusion. He seems inclined to admit two kinds of consciousness, correlated with the central changes. He frequently contends that he can not agree "with the arbitrary and dogmatic exclusion of the motor zone from the sphere of consciousness."[1] And although with reference to Prof. James' discussion of the sense of effort,[2] he says, "I fully admit the destructive character of his arguments as opposed to the introspective and circumstantial evidence of a central feeling of innervation,"[3] yet he maintains that there is experimental proof of the existence of a sense of effort which is "a subjective concomitant of the outgoing nerve-current."

Such an account of what is involved in the central changes seems to us rather obscure and inconclusive, besides being inconsistent with Waller's theory of the cortical centres. He holds that there are neither motor nor sensory centres.

> "Any 'motor' or discharging centre must also be a 'sensory' or receiving centre; it must be excited as well as excite. Any sensory centre must also be motor, directly or indirectly; else we could have no objective tokens of sensation; every centre, whether called sensory or motor, is *terminus ad quem* as well as *terminus a quo*."[4]

With such a conception of the cortical centres, it seems to us that Waller, to be consistent, should be as decidedly opposed to any theory of two kinds of consciousness as he is to the theory of two kinds of brain centres. We may grant all that he asserts as the result of his experiments with regard to the existence of central elements in effort and fatigue, and all that he maintains with regard to the sensori-motor nature of the cortex; but instead of seeing that there is any contradiction here of the interpretation of effort given by Wundt and Münsterberg, the two assertions just stated and the preceding quotations from Dr. Waller's articles are irreconcilable, except from Wundt's standpoint.

II. *Sensory and motor, or sensori-motor cortical centres.* The question whether the function of the cortex is sensory, motor, or sensori-motor, is as important with regard to the physiological basis for the theories discussed in this chapter as was the analysis of the meaning of "motor," "ideas of movement," etc., for the psychological investigation of the theories. In the psychological analysis we found that much of the apparent opposition arose from an obscure and diver-

[1] "Principles of Psychology," II, 449-518.
[2] *Brain*, 1891, p. 241.
[3] *Brain*, 1892, p. 351.
[4] *Brain*, 1892, p. 352. See also pp. 342, 343.

gent use of the same terms. The same phenomena can be described as motor when objectively regarded, and as sensory when subjectively regarded. The only ground, then, left for the controversy was the hypothetical distinction of sensory and motor in the physiological processes of the cortex. If there is no such distinction, if Waller's description of the cortex as essentially "sensori-motor" is true to the facts, then the sensory and motor controversy must come to an end. We should combine in one the observations and the verified inferences contributed by writers on both sides, and then turn to discussions of attention from some other standpoint, in search of some explanation for the descriptive theories of the present chapter. But we are forecasting the end before the final test is applied. What, then, *is* the current physiological theory with reference to the existence of sensory and motor centres in the brain?

We consider this topic in accordance with the principles of cerebral function stated in the preceding chapter. When we speak of localization, the term is used in the sense there defined. The dispute over the cortex as sensory, motor, or sensori-motor has long been waged. The names of many of the disputants have become familiar even to the general public, and it is quite unnecessary for us to review the history of the discussions between Flourens, Goltz, Munk, Hitzig, Ferrier, Gotch and Horsley, Schäfer, Schiff, Bechterew, etc. Summaries of the controversy have been given by Waller in the article on the "Functional Attributes of the Cortex," already referred to,[1] and by Mott in a discussion of the "Sensory Motor Functions of the Central Convolutions of the Cortex," published in the *Journal of Physiology* for 1894.[2] Dr. Mott's conclusions, from his own experiments as well as from his review of the experiments of others, favor the view of the cortex as sensori-motor. "I think that clinical experience, experiment, and anatomical investigation all tend to show that these cortical cells are discharged by sensory impulses traveling up fibres which terminate in the so-called 'motor area,' and that it is not 'purely motor,' but 'sensori-motor.'" "The fact still remains that motor paralysis is greatly in excess of the sensory disturbance in many clinical cases. This may be explained by comparing the expansion of the centrifugal and centripetal fibres to two funnels. . . . The base of the efferent cone consists of trunks, from which all the branches and collaterals spring. The base of the sensory cone consists only of the terminal twigs after

[1] *Brain*, 1892, pp. 329-396.
[2] *Journal of Physiology*, 1894, pp. 464-487.

collaterals have been given off."[1] Waller's quotations from both sides of the controversy show that "the differences of view have involved matters of fact far less than inferences from facts as embodied in words."[2] In fact, he calls attention to the inferences *necessarily* involved in the use of such terms for cerebral processes. "We are in presence of a central process associated with centripetal and centrifugal processes, and we have no right to say that the centre is motor, or sensory, or both, otherwise than inferentially."[3]

Still another view of the relation of sensory and motor activities is presented by Külpe in his "Outlines of Psychology." His explanation of the rapid restoration of sensory functions in the cortex depends upon reference to physiological processes, while Mott's explanation depends upon the anatomical fact of the distribution of the fibres.

"All these phenomena (*i. e.*, of recovery of sensation and loss of movement) are evidence that the specific function of a nervous excitation is as a rule dependent upon its normal place of origin: the peripheral organ in the case of sensory nerves, the central in the case of motor. It would seem, therefore, that we may compare the unknown processes of the sensory centres with the known phenomena of the motor periphery, and attempt in this way to gain a more definite idea of their nature. We must then correlate a particular sensation . . . with the excitation of a larger or smaller cortical area, according to the range of the peripheral stimulation, . . . and lastly, we may designate the state of the nervous substance, in which the various cortical areas are capable of reproduction,—*functional disposition*. Just as the piano player uses hands and fingers for the most varied combinations of movements, so the same parts of the cortex may be concerned in very different forms of excitation."[4]

This brief review of the physiological basis for the distinction of sensory and motor centres indicates that the distinction is not by any means so definite as sensory and motor theories of attention have made it. Both on physiological and psychological grounds, it is more correct to speak of attention as sensori-motor than as sensory or motor. Even if, however, separate areas are assumed for sensations and movements, so that the two are anatomically distinguishable, it is universally admitted that they are as mutually *dependent* in physiological function as sensation and movement themselves are in psychological analysis. Attention *must be* sensori-motor: sensory in its contents, motor in its mani-

[1] *Op. cit.*, pp. 480, 469. A diagram accompanies the last passage cited, and makes clear the meaning of the somewhat obscure statement.

[2] *Op. cit.*, p. 346.

[3] P. 342.

[4] "Outlines of Psychology," tr. p. 223.

festations, sensori-motor in its whole process. In the acceptance of this opinion the sensori-motor controversy is ended so far as we are concerned. But we have still reached no explanation of attention, no true definition of it. We have a description of its phenomena—subjectively in sensations, objectively in movements,—and we refer both to one general, continuous, physiological process. But this process accompanies all consciousness, and sensation and movement are present in every phase of consciousness. We must look further for the peculiar physiological processes, and for the psychological conditions which are the basis of attention itself.

CHAPTER III.

The Explanatory Theories of Attention.

The theories of the preceding chapter have been referred to as "descriptive"; those of the present chapter we call "explanatory." We have alluded to many facts and observations of attention which we may classify together as the outward expressions of the attention. We have also passed in review the reasons for characterizing attention as a sensory, a motor, or a sensori-motor process. But we have not yet been told what attention *is*. The theories have been either too narrow, defining it in terms of its secondary phenomena, or else they have been too broad, characterizing it in terms that apply to consciousness in general. The present chapter, however, is devoted to theories that do ascribe a specific function to attention. We have the essential, constitutive elements distinguished from the secondary, attendant phenomena, and the relations between the two are carefully analyzed. We have, thus, a definition of what attention is, and an explanation of its contents in the sense of a reference of all contents to fundamental principles of psychology and physiology.

There have been three types of explanatory theories. Attention is regarded (1) as essentially a facilitation of ideas; (2) as an inhibition of ideas; and (3) as both facilitation and inhibition.

I. *Theories of attention as a facilitation of ideas.* G. E. Müller is often referred to as the chief representative of the first type (*e. g.*, by Külpe). From his statement of the thesis of his monograph "*Zur Theorie der sinnlichen Aufmerksamkeit*," we should expect to find him in the third class, with those holding a mixed theory of attention. His thesis maintains that "the capacity for acting on the mind which is possessed by certain physical processes in the central organ, may

be modified by the activity of the sensible attention; they may be increased or diminished, or wholly destroyed."[1] If we look only at Müller's discussion of the physiological changes involved in attention, we must admit that he investigates both sorts of modifications mentioned in his thesis, — the inhibitory as well as the facilitatory. Külpe's criticism that Müller fails to discuss the phenomenon of inhibition ignores such passages as that extending from page 105 to page 110, as well as minor references scattered through the work.[2] On the other hand Müller certainly lays far more stress on the reinforcing than on the inhibiting function of attention. In fact, when he is confining himself to the psychological analysis of attention, he does almost entirely ignore all changes except those which he describes as reinforcements of a sensation or idea. Hence, we shall concern ourselves here only with those parts of the theory which maintain that attention is a facilitatory process.

We may neglect Müller's presupposition of "a mind that absorbs a part of the energy with which a nerve acts upon it for the production of an equivalent of mental activity."[3] His statement of his observations and most of his conclusions can easily be translated into the terms of a psychophysical theory.

In the summary of his discussion of voluntary attention, Müller mentions three ways in which attention reinforces its object. "We have found that the activity of the voluntary sensible attention consists, in many instances, in the effort to reproduce earlier sensations; in other cases, it may be evident merely in impulses imparted to motor nerves, and resulting in the adaptation of a sense organ;" this adaptation, in turn, may react on the central organs, reinforcing the idea through associations.[4] We have, then, an adaptation of the sense organ to the stimulus, a facilitation of the excitation corresponding to the stimulus in the central nervous system, and correlated with these the recollection of similar ideas previously experienced.

The brief statement just given includes what is of greatest theoretical importance in Müller's discussion. The most valuable part of the work—the application of these principles to a very large number of concrete cases, — we cannot dwell upon here. Müller's examples include references to Helmholtz, Wundt, Volkmann, G. H. Meyer, Lotze, and many others hardly less noteworthy. But his conclusions

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[1] "*Zur Theorie der sinnlichen Aufmerksamkeit*," Leipzig (1873), p. 1.
[2] *E. g.*, pp. 14, 67, 71.
[3] *Op. cit.*, p. 70.
[4] *Op. cit.*, p. 103. See also p. 48.

are not systematically worked out, and his presupposition of a "mind" acting upon a "sensorium" has prevented his recognition of certain problems in attention which are the natural outcome of his theory, *e. g.*, the question of our knowledge of the difference between increased intensity due to the stimulus and increased intensity due to the attention. Such problems will recur when we reach the discussion of the third type of the explanatory theories. Meanwhile we must examine the second type of theory, that which is directly opposed to Müller's, and which, both logically and chronologically, is the next in order for our investigation. The two chief representatives of the theory that attention is essentially inhibitory are Wundt and Külpe.

II. *Theories of attention as an inhibition of ideas. A: Wundt's theory.* Wundt analyzes the whole process of attention into the following subordinate processes:

"(1) Increase of the clearness of a definite idea or group of ideas, accompanied by the feeling which is characteristic of the whole process from the beginning; (2) Inhibition of other available impressions or memorial images; (3) Sensations of muscular strain, with the sense feelings which belong to them and which intensify the primary feeling; (4) Intensification of the sensory contents of the apperceived idea by these strain sensations through the medium of associative co-excitation. Only the first and second of these four part-processes are essential elements of every act of apperception. The third may be of very slight intensity, or even entirely wanting; the fourth is demonstrable only in cases where the third, of which it is a secondary consequence, attains a certain duration and intensity."[1]

In our attempt to summarize Wundt's theory of attention, we shall take up the constitutive factors (part-processes) in the order just given. It is not the order in which they are discussed in the section on *Aufmerksamkeit und Apperception*, but it is evidently the order of their importance according to Wundt's own estimate.

The first process is "an increase of clearness of an idea with an accompanying feeling of activity." Clearness is distinguished from intensity.

"Since the intensity of the sensations which make up an idea exercises an undoubted influence on its clearness, these two concepts are often confused. But in strictness we can attribute intensity only to the sensation-elements, not to the idea itself. . . . . The essential difference between the clearness of an idea and the intensity of its sensation-components is shown above all in the fact that an increase and decrease in clearness can occur without a simultaneous increase and decrease of sensation-intensity. . . . If a continuous stimulus is allowed to act on a sense organ, even under conditions which preclude fatigue of the organ, it is more or less impossible to apperceive it continuously with the same clear-

[1] *Phys. Psychologie*, IV*te Aufl.*, II, 274.

ness and distinctness. A constant change in clearness is noticed, and this change appears to the subject as a process which is different from any objective oscillation in the intensity of stimulus. . . . If clearness and intensity are so entirely different, then the concept of the stimulus-limen acquires a double significance. As limen of *intensity* it means a limen of *consciousness*, since the entrance into consciousness (perception) of an idea depends upon the intensity of its sensory contents. The limen of *clearness* is something different; it is a limen of *attention* or *apperception*. Only impressions which lie above the limen of intensity can transcend the limen of apperception. But, if this is to happen, the subjective function of attention must also be discharged. . . . . We have proved, then, that an impression may oftentimes become clearer without growing stronger, and vice versa, and that the two processes are subjectively distinct. Still, this does not prevent their exerting a certain influence on each other. . . . An intensive impression, provided that there are no special dispositions present to oppose it, is ordinarily apperceived more strongly than a weak impression. But undoubtedly certain influences may be exerted in the contrary direction as well; as . . . when one makes an effort to recollect or to imagine, and tries to keep his ideas in consciousness at the highest possible degree of intensity. . . . Many persons, it would seem, are successful in increasing the clearness of such images, but are absolutely unable to increase their intensity to any marked degree. . . . Increase of clearness always precedes increase of intensity, and the latter always takes place more slowly, and appears as the accompaniment of strong strain sensations,—the character of the muscular excitation naturally being in exact correspondence to the form of the apperceived idea. . . . It is extremely probable that the intensification of sensations is a *secondary* effect, which may be—but is not necessarily—produced by certain concomitant phenomena of attention."[1]

Wundt's statements of the distinction between clearness and intensity have been quoted at length not only because the distinction is of such importance to his own theory, but also because the relation of the two phenomena is one of the most vital questions in any complete theory of attention. We shall return to this point later. We have now to consider the first part-process from its second point of view, *viz.*, the feeling of activity which Wundt has associated with the increasing clearness of an idea.

It must be noticed that "feeling of activity" is frequently used as a general term, including the negative feeling of passivity as well as the positive feeling of activity; for Wundt has two classes of attention, the passive and the active, and if we use the *Thätigkeitsgefühl* as the characteristic feeling accompanying increased clearness of an idea, it must be remembered that the term also covers the feeling present in passive apperception. To simplify matters we shall consider the term in this section only in its restricted and more ordinary meaning.

[1] *Op. cit.*, II, pp. 271-274.

Wundt states that the feeling can not be analyzed or defined. This remark is constantly quoted by his critics. Some misunderstand Wundt so far as to maintain that he is introducing a metaphysical concept. Wundt himself, however, distinctly states that he is speaking of "empirical facts and not of any metaphysical conception."[1] Other critics, proceeding on the presupposition that sensations are the only elements of mind, object to Wundt's *Thätigkeitsgefühl* because it has no content; and, thus condemning his introduction of a "feeling of activity," assume that his whole theory has been demolished by such condemnation.

Criticism of this sort is crude and ill-considered. It is true that certain passages in Wundt's writings give some basis for it, but the discussion as a whole cannot be overturned by the demonstration of a few small inconsistencies. We are bound to admit that there are inconsistencies in Wundt's account of the "feeling of activity," but they have been exaggerated, and, moreover, the whole theory of attention has been made to depend upon the validity of this one factor. We hope to show that the "feeling of activity" is not essential for the logical coherence of the rest of the theory in general. But before we take up its relation to the discussion as a whole, we should notice carefully what Wundt himself has to say in defense of the much criticised *Thätigkeitsgefühl*. He first characterizes it as a feeling *preceding* the apperception or the increased clearness of an idea. "And we think of the ideas associated with this feeling as originated by ourselves. Other ideas are accompanied by the feeling of passivity. This latter feeling comes into existence *simultaneously* with its associated ideas, so that the idea seems to arise without any activity on our part."[2] But the feeling of activity does not merely precede, it may also persist, even when an idea has reached perfect clearness in consciousness. In this connection Wundt indicates that the feeling itself cannot take sole possession of consciousness. In the interval that he refers to as "preceding" that of the full clearness of an idea, the feeling accompanies the appearance of a series of memory images and their more or less complete fusion with the idea attended to.[3] "At the same time definite sensations take part in the process,"—the strain sensations of Fechner; and again he says of the *Thätigkeitsgefühl*, "As soon as we reflect upon it, it is referred by us to the whole disposition (*Anlage*) of consciousness ordinarily contained in the word 'I.' But if we analyze this reflection, the feeling is brought

[1] *Op. cit.*, II, p. 284.
[2] II, p. 266.
[3] II, p. 270.

into connection with definite, complex motives, *i. e.*, with complicated associations of ideas involving strong feelings. I find that all these feelings and motives are empirically given facts."[1]

To repeat a former statement,—it seems to us that there *is* some justice in the criticism of Wundt's account of the feeling of activity as confused. He describes the feeling as unanalyzable, and yet gives it a diversity of contents. This is partly due to the fact that at one time he is regarding it from a purely subjective point of view; at another time, from an objective point of view, he sees its mechanism,—its physiological expression. Another reason for the confusion is due, in our opinion, to the indefiniteness of the whole system in its treatment of the feelings (*Gefühle*). After maintaining that there are only two affective qualities, pleasantness and unpleasantness, Wundt refers to several concrete, *unanalyzable*, feelings. The *Thätigkeitsgefühl* is such a feeling, accompanying apperception. But apperception is analyzed into conscious processes that every one must recognize, whatever name one may choose to give them. Wundt, in his last edition, does not maintain the existence of an activity of apperception as an immediate fact of consciousness, in any sense that excludes its analysis into changes in sensations or ideas with accompanying variations in their affective tone. The activity itself is an *inference* from the facts observed. "The phenomena of the connection of ideas in active attention cannot be accounted for by reference to the so-called laws of association alone, but the influence of more remote acquisitions and dispositions of consciousness makes itself felt here."[1] Later in the same passage Wundt refers to the apperceptive process as one in which "the whole past experience of consciousness influences the new impression."[2] And yet, elsewhere, he does maintain that apperception finds immediate expression in consciousness in one way—in the form of the "feeling" of activity, and this feeling is unanalyzable!

To understand what Wundt includes in the *Thätigkeitsgefühl*, why he makes it a factor in attention, and what its relative importance in the system is, we must digress for a moment, and refer briefly to his conception of the feelings in general. The digression is justified by the fact that even those who are in accord with Wundt's system as a whole often misapprehend his treatment of the "*Gefühle*" and the affective side of consciousness. It seems to us that Wundt

[1] II, p. 284.
[2] II, p. 284. See also "Human and Animal Psychology," tr., p.252.

regards the feelings from two points of view: that which considers the components or the sources of the feeling, and that which considers the very nature of the feeling itself. In many cases we are able to distinguish the components of a feeling, but the feeling itself is a *unitary* state. It is not the mere sum of its components, but their product or resultant,[1] and it is, therefore, in a sense unanalyzable and indefinable. But besides these qualitative distinctions in compound feelings, there are qualitative differences also in the most elementary feelings. Pleasantness and unpleasantness are the most common and extensive of the variations in quality, but Wundt does not hold them to be the only affective elements. "Both are qualitative states, and each may pass over into the other through a zero or indifference-point. Each of them may go through extremely different degrees of intensity or may be present in very many shades of qualitative difference."[2]

And after an account of the relation in many sense departments between the intensity of the stimulus and the feeling of pleasantness and unpleasantness, he adds that "in sensations of the two higher senses there are contrasting feelings analogous to pleasantness and unpleasantness, but not to be directly characterized by these terms."[3] Among the feelings which are connected with sensations of sound and light, every feeling has its opposite, as is the case with all classes of feelings, but the contrasting states cannot here, any more than in the case of the lower sensations, be characterized simply as pleasant or unpleasant.[4] Wundt accounts for this difference in the qualitative variety of the feelings accompanying sight and hearing by reference to "factors which have their source in the development of consciousness," due to inherited dispositions and to individual activity.[5] But this account of their origin does not make them any the less "elementary" in consciousness as we now know it.

The feelings can never exist alone. They must be con nected with a sensation or with an idea. As dependent upon ideas of sometimes very great complexity, the æsthetic, intellectual and moral feelings may be said to be complex, but as we have already noticed, Wundt emphasizes their unitary character and their absolute qualitative distinction from one another and from their component elements.

We have nothing to say here with regard to the truth or un-

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[1] "*Phys. Psych.*," II, p. 498.
[2] I, p. 555.
[3] II, p. 558. See also p. 561.
[4] I, p. 570.
[5] I, p. 590.

truth of the statements with which such a theory of "feeling" must stand or fall. They are matters of introspection and analysis quite outside of our province. We only wish to point out that feeling and emotion may be interpreted in very different ways without affecting Wundt's theories of the other phases and processes of consciousness. Wundt himself has given us an instance of one such change in his modification of the *Innervationsgefühl*. The *Thätigkeitsgefühl* could be interpreted in the same way — analyzed and defined, *e. g.*, in the way in which Külpe describes it,—and yet the rest of Wundt's theory of attention would be left undisturbed by the change. · Indeed it seems to us that the whole discussion of apperception in the fourth edition of the "*Psychologie*" carries one on to a position where an "unanalyzable" feeling of activity has no place, and that Wundt himself has shown more clearly than anyone else that it is not necessary in order to explain the facts. It would have been, we think, more consistent with his own system, and more true to the facts, if he had recognized the "feeling of activity" as a feeling in the ordinary sense of the word—a concrete, complex phenomenon, including muscular sensations; the idea of activity, or of a self, reached by inference from the facts of consciousness; and, at the same time, the affective tone accompanying these sensations and ideas. Such a conception of the feeling of activity would complete the work Wundt has already carried so far, *i. e.*, an analysis of attention that shall combine the psychological data of the neo-Herbartian theorists and the physiological observations of the empiricists. On the one hand, Wundt records the psychological facts freed from any metaphysical implications; on the other hand, he proceeds on the assumption that physiological changes accompany all the conscious processes, and no theory is accepted unless it agrees with both series of facts so far as they are known.

The second factor in Wundt's account of the process of attention is that with which we are specially concerned in this chapter; attention is an inhibition of all impressions and memory-images except those attended to. This inhibition is the function of the frontal lobes—the so-called "apperception centre." The apperception centre is intricately connected with the other centres in the brain. An excitation passes from a sensory centre on to the apperception centre, and outwards along the motor path. The effect of the entrance of some particular excitation is the inhibition of the passage of all other excitations. In accordance with the law of the relativity of consciousness, the idea corresponding to the uninhibited excitation gains in clearness in proportion as it is freed from competition with other ideas. As the excitation

completes its course by the production of movements which are in harmony with the apperceived idea, these movements give rise to sensations akin to the idea. The motor adjustment causes "associative co-excitation," which may, and ordinarily does, intensify the original idea. Wundt describes this indirect reinforcement in the third and fourth divisions of his definition of the attention, and in that connection refers to it as a very frequent factor in attention, although not absolutely essential. If, however, such intensification of an idea is a secondary effect, we must ask whether there is any *direct* reinforcement of the sensory centre from the apperception centre. Wundt says:

"According to the general principles of central excitation, we could as well think here of an excitatory as of an inhibitory effect. . . . . But in view of the fact that increase of clearness, with which we are here concerned, is quite distinct from increase in the intensity of a sensation, the first of these suppositions has evidently little probability. In order to carry it out, we should have to conceive of the excitatory effect of these central processes as qualitatively different from other excitatory effects. This would contradict the principle of the uniformity of elementary functions, and it is also in opposition to the fact that the motor effects of the centre *A C* are just like other motor excitations. On the other hand there is nothing to oppose our regarding the effect of the apperceptive centre to be inhibitory. This inhibition, indeed, is not to be thought of as a *destruction* of certain excitations in sensory centres. It simply consists in the fact that in consequence of the excitatory effect of the signal stimulus, the entrance of other signal stimuli to the apperceptive centre is prevented. This fully harmonizes with our experience that the focus of attention narrows with its degree, and that, after repeated attention to certain ideas, the focus can extend so as to include more of these ideas."[1]

It is hardly necessary for us to state that Wundt is not maintaining the existence of an "organ" or "centre" of apperception in any form contradictory to the principles of localization presented in the first chapter. Wundt's account of the sense in which he uses such terms is so explicit that we need only refer to it without quotation.[2] He asserts nothing more than that the functioning of certain parts of the brain "is a necessary part of the physiological processes which accompany the intellectual functions."

It seems strange that after such an explanation of his position, Wundt should be accused by Münsterberg, Bastian and others of making attention a "faculty" and localizing this faculty in some part of the brain. Just as Wundt maintains that there is no "consciousness as such," but that there are "conscious states," or "conscious states and processes,"[3]

[1] *Op. cit.*, p. 276. See also p. 286.
[2] I, pp. 224-228, and II, p. 285.
[3] I, p. 226; II, p. 255; and "Lectures," pp. 236, 238.

so he would affirm that there is no "attention as such," but that there are "ideas attended to." And the changes in the ideas which constitute what we call "our attending," are accompanied by physiological processes; primarily, by processes in the frontal lobes; secondarily, by processes in other parts of the cortex, in the sense-organs, or in any part of the periphery. This is practically Wundt's position, and we do not see that it is touched by any criticisms directed against the localization of a faculty.

One difficulty, however, we do feel in an attempt to follow out Wundt's account of the action of the apperception centre. The description of the inhibitory action of the frontal lobes is very vague. A "signal-stimulus" enters, and all other stimuli are inhibited. How are we to conceive of this inhibition? "Inhibition" is itself an excitation, and some physiologists refuse to use the term except in referring to the action of certain excitations upon muscles. If, however, the term may properly be used in speaking of the interaction of excitations in the nerve substance, there is still much obscurity to us in the application of the process in order to explain the facts of attention. Wundt and Külpe pass over the matter too lightly. They should recognize the difficulties still left in the way, and either answer them, or state that they are unanswerable in the present state of our knowledge.[1] In fairness, however, we must recall the fact that they do refer to their use of inhibitory processes as somewhat hypothetical. The difficulties that arise are not merely such as are due to ignorance. The nature and extent of inhibition are subjects of investigation among neurologists at the present time; and occasionally, as we shall see later, the inhibitory nature of the frontal lobes is denied.

Before leaving the topic, we take up two questions raised with reference to Wundt's theory in a recent treatise, "*Zur Theorie der Aufmerksamkeit*," by H. E. Kohn. The consideration of the two objections will clear away certain misapprehensions that might obscure Wundt's real meaning.

The first objection is a charge of inconsistency in Wundt's references to the action of the frontal lobes. Dr. Kohn cites passages in which Wundt speaks of the action as "*eine hemmende Wirkung*," and others in which he speaks of it as "*erregende*."[2] But the apparent inconsistency is due to the two uses of "*erregen*." It may be defined so as to include inhibition, or so as to stand in contrast with it,—either referring to excitation in general or to adjuvant excitations. With

[1] See below, p. 41 and note.
[2] "*Zur Theorie der Aufmerksamkeit*," 1895, pp. 42, 43.

this in mind there seem to us to be no contradictions in the passages quoted.

The other objection raised by Dr. Kohn maintains that Wundt's theory fails to account for any increased clearness in an idea. "The 'signal-stimulus' has only this effect, that the energy of the apperceptive centre released by it merely inhibits other 'signal-stimuli.' We do not see what the original stimulus gains from the signal-stimulus which calls it forth, and from the apperceptive process which it excites. If the stimulus reaches the sense centre, it experiences absolutely no change through the apperceptive process."[1] Wundt's answer to this would probably be that clearness and intensity are known relatively, not absolutely. If competitive stimuli are absent, the stimulus present is relatively clearer, and it is also the more likely to be intensified by the "secondary effects" already described. Külpe states the case rather more clearly than Wundt: "If we accredit attention with inhibitory functions only, the peripheral and central excitations and their relations, if they can secure free and unimpeded recognition in consciousness, produce all the phenomena which characterize the state of attention, of their own power and by their own laws; and the function of apperception consists simply in the suppression of competitive activities."[2]

We have discussed the first and second of the four part-processes which Wundt includes in attention. In the course of the discussion we have had to refer occasionally to the last two processes, since they are the source of the increase in intensity frequently accompanying attention, and since this intensification is the ground for the consideration of attention as a reinforcing process. Partly because they have been already referred to, and partly because they are recognized to be of more or less importance by every theory of attention, we need give no further consideration here to "the muscular strain sensations, with their attendant feelings," or to "the intensifying effect of these strain sensations on the sensory contents of the apperceived idea through associative co-excitation."

B: Külpe's theory. Külpe's theory of attention, in so far as the importance of inhibition is concerned, is much the same as Wundt's, but it discusses more fully the relation of attention to other processes, it emphasizes more distinctly certain factors, it rejects Wundt's first factor, and it recasts Wundt's material into quite a different form. We shall only

[1] *Op. cit.*, p. 42.
[2] "*Grundrisse der Psychologie*," 1894, § 76, 4; Eng. tr., p. 444.

consider these differences in so far as they may possibly offer some further explanation of attention.

The best "explanation" is often a well-ordered description of the facts in their observed relations. Hence, we consider first the order of Külpe's treatment. After defining attention as a *state of consciousness*, he describes its effects, the phenomena which accompany it, its conditions, and lastly the theory of attention, "the determination of the essential factors in attention itself."[1] The term "effects of attention" is applied to "the changes which conscious contents undergo in the attentive state."[2] These changes are an increase in sensitivity and sensible discrimination; greater liability and fidelity of reproduction; the disappearance of feelings when attended to as such; a greater ease in the analysis of fusions; the concentration of consciousness; the more rapid rate of perception or reaction.

The section on "the phenomena of attention" describes the phenomena regarded by Ribot as constitutive elements, but by Külpe as concomitants, characteristic, but not essential to attention. Some of these are changes in respiration and in other bodily movements, central and peripheral hyperæmia, centrally excited sensations, adaptation of the sense organs, and sensations of strain.

By "conditions of attention" Külpe means "the incentives which lead to the appearance of particular contents in the attentive state."[3] The conditions are divided into two classes, external and internal. The external include those motor and sensory changes which direct, or tend to direct, the attention upon certain impressions. The internal conditions are: the affective value of the impression, and the relation of the impression to the psychophysical disposition; including (α) the associative relations of the impression to the ideas already in consciousness, (β) its relations to the materials of reproduction, and (γ) the relative vacancy of consciousness.

The last section, on the "theory of the attention," is "a determination of the essential factors in attention itself." A large part of the section is negative, presenting a refutation of three views of attention: that which regards it as nothing else than an increased intensity of sensation; that which maintains that attention is "an emotive state translated into terms of motor energy" (Ribot), and that which explains attention in purely psychological terms. As the criticism of this last view leads up to Külpe's own theory, we must spend a little time upon it.

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[1] *Op. cit.*, § 76, 1.
[2] § 73, 1.
[3] *Op. cit.*, § 75, 1; tr., p. 437.

"An explanation of the attention in purely psychological terms, such as has been attempted, *e. g.*, by Herbart, obliges its adherents to ascribe a determining influence to the unconscious as a psychical process, if not to make it the one primal force in mind. For there can be no doubt that the most essential conditions of the origin and maintenance of attention must be sought outside of consciousness. The evidence on this point is overwhelming. The force of attention is wholly unintelligible on any other hypothesis; it is a frequent experience that the incentive to the preference of a content by apperception does not come to consciousness; and individual variation of the capacity of attention in normal subjects, as well as the differences observed in pathological cases, put the assumption of definite physiological conditions of a central character beyond the range of question."[1]

In accordance with the opinion just quoted, that the most essential conditions of attention must be sought outside of consciousness, Külpe maintains that the essential function of attention is to inhibit. Neither the mechanism of inhibition nor the inhibitory process, however, is conscious.

Our chief objection to Külpe's treatment of the attention is his inconsistency in defining attention as "a state of consciousness," and yet refusing to recognize any factor as *essential* except the unconscious, purely physiological process of inhibition. Every psychophysical theory involves some physiological correlate, but that this process *constitutes* attention, and that all the psychological phenomena are secondary, we deny. Külpe, however, does not arrive at his conclusions without consideration of the psychological changes that have been regarded by others as essential factors of attention, and before any elaboration of our objections to his position, we shall quote from his own argument against theories of the psychological type, selecting, above all, those passages which criticise Wundt's opinion that increased clearness of an idea is an essential.

"Introspection discovers nothing really new in attention, nothing which is characteristic of the process as such. . . . When we find the increased clearness of a perception or the increased vividness of a sensation or conscious concentration upon one or two definite ideas, interpreted as functions of attention, we must reply that while all these changes appear in the attentive state, they are always changes in the conscious contents, and cannot be conceived of as separated from them. It is, therefore, altogether impossible to add all these changes together and account them a specific process, and to give this process the name of attention in contradistinction to the particular contents; for every item in the sum proves to be . . . . nothing more than a purely quantitative process, which may be set up in consciousness by other means than that of attention. The reader may incline to find a possible exception to this in the clearness of the quality, difference, etc., in the state of attention. For clearness or distinctness is certainly not identical with the intensity of an impression. . . . Now distinct-

[1] *Op. cit.*, § 76, 3; tr., p. 443.

ness is partly referable to conditions in the sense organ. . . . . But, this apart, we mean by the term simply the relatively most favorable apprehension of an impression, as expressed in a relative maximum (α) of its discriminability from other contents, and (β) of the liability of reproduction of its attributes. It follows, of course, that clearness or distinctness is not a new character, which can be detached from the particular contents to which it is ascribed, and constituted the predominant factor in attention."[1]

There are two lines of thought here. With one of them we entirely agree; *i. e.*, with all that Külpe has to say of attentive changes as "purely quantitative," introducing nothing that can be "conceived of as separated from the conscious contents," or considered "in contradistinction to them." We agree also with his analysis of clearness. But it does not follow that clearness cannot constitute the predominant factor in attention; nor is it true, in our opinion, that clearness can be "set up in consciousness by other means than that of attention." Külpe is ignoring here the facts of "passive attention." All that he says in this passage only goes to show that attention is a relative state, and is always present in some degree in our conscious life. Even in that dim region which Wundt refers to as lying above the "limen of perception" and below the "limen of apperception," it does not seem to us that clearness, distinctness, attention, or, in other words, apperception itself, is wholly absent. There is a difference in degree, which appears to be a difference in kind when considered in its relations to memory, language, or practical and experimental uses. But when we consider conscious facts apart from such relations, we cannot see that there is any absolute separation in consciousness of consciousness as such, and attentive or distinct consciousness.

Our own modification, then, of Külpe's theory would be the elimination of the increased clearness of a perception, in its two-fold definition quoted above, from the section on the "effects of attention," and its introduction into the section on the theory of the attention as a constitutive factor of the state. The question as to whether the essential physiological factor is inhibition must still be left open, at least until the evidence in favor of other theories has been more carefully considered.

III. *Theories of attention as combined facilitation and inhibition.* We have reviewed the theories which attempt to explain attention as a central reinforcement, and central inhibition of ideas. We now turn to a theory which holds that neither of these alone gives an adequate explanation of the facts, and maintains that attention must be both a reinforcement and

[1] *Op. cit.*, § 72, 2; tr., pp. 424, 425.

an inhibition. The chief exponent of this theory is Exner. He enters into the subject from a physiological standpoint, with an intimate knowledge of the nervous system and its processes. It is impossible to do justice here to Exner's construction of a physiological basis for his theory of attention. We have quoted in our introductory chapter the evidence he adduces in favor of the new concept of *Bahnung*. But his systematic application of this and of more familiar concepts cannot be properly valued without a careful review of the work as a whole. Obscurities in our summary of his theory will be removed, we think, by reference to the chapters preceding that on attention, in which Exner describes and schematizes the nerve processes correlative to the more elementary processes of conscious and unconscious movement. We, however, can attempt to present here only those portions which stand in immediate connection with the explanation of the peculiar and distinguishing characteristics of attention itself.

Of all the sections preceding the chapter on attention, that which deals with reaction times (pp. 156-162) seems to us to stand in closest connection to the subsequent definition of attention, and as a preliminary to the citation of that definition, we quote the author's explanation of the motor type of reaction.

> "There can scarcely be a doubt that this form of a typically voluntary movement is to be referred to the fact that the intention (*Willensintention*) to execute a definite movement as quickly as possible upon the appearance of an expected sense stimulus, depends upon a change which the cortical processes bring about in the relations affecting the excitability of sub-cortical centres. . . . The reader will at once recognize the similarity between the relations here described and those already mentioned in the account of *Bahnung* [quoted in our first chapter] and of the regulation of voluntary movement by ideas [pp. 151-154]. In fact it was through the investigation of reaction times that I was led to believe in the existence of the phenomena which I have described above under the name of phenomena of "facilitation," and for which I later found proof in experiments on animals."[1]

If we bear in mind, then, that voluntary action is to be thought of as "a change which cortical processes bring about in the relations affecting the excitability of sub-cortical centres," and that this change is referred to as either a facilitation or an inhibition, or both, we have the two most important ideas involved in the definition and explanation of attention. Exner states the definition in these terms:

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[1] "*Entwurf zu einer Phys. Erklärung der Psychischen Erscheinungen.*' p. 158.

"We have shown that an act of will can bring about changes in our nervous system, the effect of which is that an excitation now follows a path *A*, and now, when a different change has been voluntarily set up in place of the former, takes the path *B*. The changes thus set up have the character of states. I see in this interaction of different parts of the nervous system the essence of what we ordinarily call attention."[1]

Exner's schemata are so serviceable in the course of his explanations that we reproduce here the "schema for the demonstration of the effect produced by the direction of the attention to sense-impressions." And we quote from the context the passages that give the substance of Exner's explanation and application of the schema.

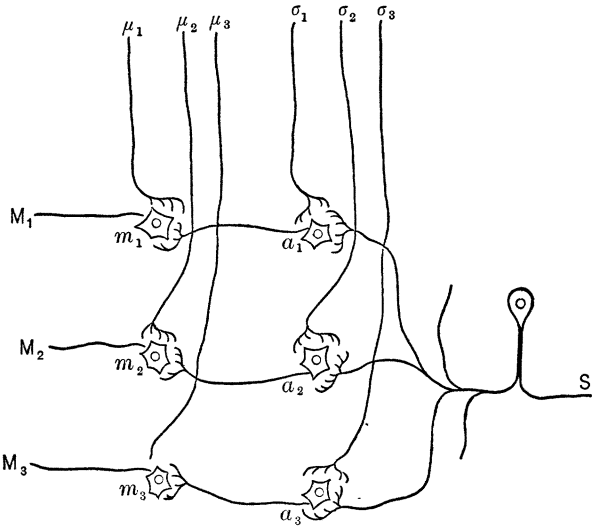


Fig. 48.

"If we apply the diagram to the case of the motor form of reaction when only the group of muscles represented by M_1 is to contract, then we must think of the cell m_1 as being charged through μ_1; and perhaps also the cell a_1 is charged through σ_1. Then when the stimulus enters through *S* it will pass through a_1 and m_1 to M_1. In this instance the paths facilitated lie in the sub-cortical centres. Similar changes can take place in the cortex, as we shall see later."[2]

"But attention can also be applied to the stimulus entering through *S* without being connected with a tendency to movement, as when, for example, some particular part of an object interests us. The physiological state of attention then consists in a charging of the *a*-cells through the *a*-fibres. [These *a*-fibres will carry excitations both to and from the cortex: *e. g.*, pp. 153, 155 and 164.] A feeble excitation by way of *S* will then bring these cells to the point of

[1] *Op. cit.*, p. 163.
[2] P. 163 (paraphrase).

discharge; and if the charging is still kept up through the a-fibres, the a-cells will remain in continuous excitation until they, or the central terminations of the a-fibres, or both, are exhausted, — in which case we speak of fatigued attention."[1]

"But although attention *may* thus be exclusively sensory, 'it will probably be found, in accordance with the diagram, that there must be a *tendency* to movement in sensible attention, since the m-cells receive stronger impulses than when the a-cells are not charged; impulses which are stronger before as well as at the time of the entrance of the sensation. . . . Introspection shows that at least those muscles whose region of innervation is nearly related to the sensory region concerned can scarcely be kept at rest if the attention is directed to the corresponding sensations.' "[2]

Examining the physiological phenomena of attention more closely, we find that "whether the attention is turned to sense-impressions or movements or memorial images, it always causes certain paths of the nervous system to become specially practicable *(fahrbar)*, and to remain in this state a longer or shorter time; and, furthermore, reduces the conductive power of a great number of other paths. The more intensive the attention, the lower is the excitability of these other paths. . . . I will designate this total state of the system by the term *Attention* [not *Aufmerksamkeit*], and accordingly speak of attentional inhibition and attentional facilitation. The dividing line between them may have to be drawn at very different points in different sense departments if we choose to venture on comparisons of department with department, and will certainly vary from case to case with the disposition of the whole nervous system."[3]

Exner's definition of attentional inhibition is rather perplexing. He says, "By the term attentional inhibition, I mean a state of the centres somewhat like that which prevails in a reflex organ or in a centre subserving instinctive movements,—a centre which is stimulated to action by an adequate stimulus but prevented from acting by the will. I mean, *i. e.*, an increased tonus of the cells, in spite of which discharge is obstructed. And this region of simultaneous facilitation and inhibition may be variable."[4] Since Exner's concept of the will is a thoroughly psychophysical one, the statement quoted cannot mean what on the surface it seems to mean. The passage must be interpreted in the light of his general treatment. The last sentence of the quotation shows that here, as elsewhere, attentional inhibition and facilitation are being considered together as coöperative and complementary processes. To make the passage at all intelligible or consistent, we have to paraphrase it in the following fashion. If we attend to an object there may be a rise in the tonus of a region a. This region may now be compared to that connected with a reflex organ in which there is a permanent "facilita-

[1] P. 164.
[2] P. 164.
[3] Pp. 165, 166.
[4] P. 166.

tion" of discharge in certain directions. In both cases there is a tendency to discharge, but actual discharge will be prevented if there is at the same time an attentional facilitation of some other region. Exner might also refer here to his account of the inhibition of *motor* phenomena in the passage already quoted from pp. 163 and 164. But apparently he thinks of that as so exceptional that after once stating its possibility, he makes no further use of it. And with this exception he always refers to attentional inhibition as attendant upon and simultaneous with attentional facilitation.[1]

The most important point with regard to attentional facilitation is the relation of attention to the intensity of a sensation. As Exner says, "From the preceding explanation we might expect the intensity of a sensation to increase as the attention was more fully directed upon it. As a matter of fact, if the cell σ_1 (Fig. 48) is charged, and at the same time receives impulses through the fibre σ_1, the retroactive excitations must be greater than if σ_1 had carried no impulses to a_1. In other words, the intensity of the intercellular tetanus (see chapter II, p. 94) depends upon each of the coöperating factors."[2] We recall at once the conclusive objections against the consideration of attention as nothing else than an increased intensity of sensation so concisely stated by Külpe.[3] But Exner's further development of his theory shows that it is not touched by the objections stated by Külpe. For although he holds that attention does increase the intensity of a sensation, he also maintains that there is a recognizable difference in consciousness between the enhancement due to increase of stimulus and that which results from increase of attention. Another diagram will give his idea more exactly.

The accompanying statement is as follows:

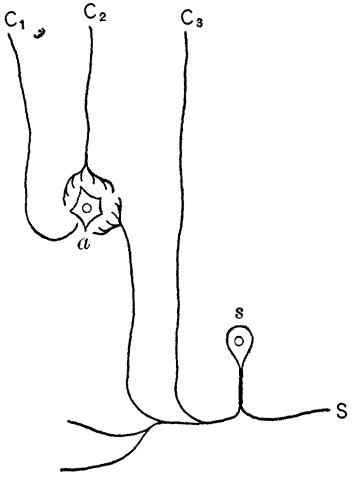
"Nevertheless, the excitations which reach the organ of consciousness will be different according as the external stimulus, or the attention, is increased. And if they are different, the two cases will be distinguished in consciousness. Let a represent a cell through which impulses of attentional facilitation, following the path C_1, flow from the organ of consciousness toward s; and let there be, running from s to the cortex, the other path, C_3, which has occurred so often in our previous figures. There will then be an intercellular tetanus established between a and s. The impulses given off from a may be diffused along several paths; we are ourselves concerned with the further path C_2, which may also give occasion to a conscious sensation.

[1] Notice that this statement holds only with regard to *attentional* inhibition. It does not refer, of course, to the purely physiological inhibitions described in Exner's second chapter, pp. 71-76.

[2] P. 168.

[3] "Outlines of Psychology," § 76, 1; tr., p. 441.

"Now when we consider the diffusion experienced by every excitation that penetrates the gray matter, due to the ramification of its paths, we cannot doubt that conscious processes may be aroused by the stimulation of S by way of C_3 as well as by way of C_2, and probably also by way of C_1. There is just as little doubt that in case of simultaneous facilitation by way of C_1 . . . the relation between the impulses flowing into the organ of consciousness by way of C_2 and C_3 is altered. There are at least no reasons for assuming that the relation remains the same, while there are many for supposing that it undergoes a change (cf. p. 58).



"In other words, if C_3 conducts an excitation of a certain intensity to the cortex, and at the same time C_2 conducts one of much less intensity, we have *one* total impression. If C_3 conducts an excitation of the same intensity as in the first instance, and C_2 another of greater intensity, then we have *another* total impression. These two impressions must therefore be distinguishable in consciousness, just as according to the modern theories of color-vision the impression of red-orange is distinguishable from the impression of yellow-orange owing to the relatively unequal excitation of the same kinds of fibres."[1]

Exner gives two illustrations to show how this theory explains the facts "in the domains of sensitivity and motility." The first instance is an application of the schema to the fact that we have a sensation of blackness when there is no excitation of the retinal elements. The second instance is an explanation of the apparent movements of objects when the motor apparatus is rendered incapable of functioning.[2]

Exner further limits the influence of attention to processes accompanied by consciousness. Sense impressions have,

[1] Pp. 168, 169.
[2] Pp. 169, 170.

therefore, been "worked over" once at least before they can be modified by attention.[1] This is one reason for the high degree of accuracy of mere "impressions," or of judgments with regard to sensations inattentively experienced.

To conclude our summary, we may quote the closing paragraphs of the chapter on attention:

"I must not omit to emphasize the fact that I do not assume the processes of attentional facilitation here described merely for cases where the attention is intensively directed upon its object. I believe, on the contrary, that no central function ever wholly disappears; that a certain degree of excitation (which I have repeatedly referred to, in contradistinction to intercellular tetanus, as the tonus of the cells) is always present; that the degrees of this tonus vary; that its magnitude is primarily dependent on excitations that run their courses along related paths; that in connection with these, phenomena of inhibition and facilitation may appear, etc. . . . . But the more intensively the attention is applied to one complex of paths, the more it is withdrawn from the rest."[2]

We pass now to a consideration of the comparative validity of Exner's theory of attention. We recognize that in any attempt to evaluate theories representing the life work of men who are recognized as authorities, we are treading on dangerous ground. Any judgment must be a merely tentative one;—indeed it can hardly be called a judgment, — it is rather an investigation of the ground of the differences in the theories and a statement of the alternatives offered in the acceptance of the one or the other.

It is evident that the fundamental concept in Exner's theory is that of coördinated facilitation and inhibition, and of these two processes we are concerned only with the member which other theories of the day reject—facilitation. Exner treats of facilitation first as a purely physiological process, and secondly as a process involved in attending. What he had to say under the first rubric we have quoted in our first chapter. We suspended comment there in order to review the theory as a whole before criticising any part of it. As we return to it now, the account of the various experiments on animals seems to us to be an account of facts known before presented here under a new name. Certainly we knew that there was interaction between nuclei connected with groups of muscles assuming a single function, or closely related functions. But these nuclei are in close anatomical connection. If *Bahnung* means simply that nerve currents in the same organ, or in organs with close and definite anatomical connections, can reinforce each other, then it is no new concept. We think the whole subject would have been clearer if Exner had stated

[1] Pp. 166, 167.
[2] P. 171.

this himself, and then given his reasons for emphasizing and further illustrating by experiments of his own facilitatory interaction of certain nuclei of the nervous system. It seems to us that he emphasizes the experimental proof of this interaction in the nervous system of animals because he wishes to extend the concept further to cases where there is no direct, intimate anatomical connection, and the facilitating process is accompanied by consciousness. To this extension of the concept he gives the name of "attentional facilitation," our second subordinate topic. Having accepted the first, the purely physiological concept of *Bahnung*, what have we to say to this extension of it to activities accompanied by consciousness?

In the first place we must recognize that facilitation in attention is as yet only an hypothesis. Exner would doubtless admit this, for he refers occasionally to the hypothetical nature of his explanations (*e. g.*, p. 87). At the same time he maintains that the hypotheses advanced are those which are in closest correspondence with our knowledge of the physiology of the nervous system. From this standpoint there are many facts to render probable the hypothesis of attentional facilitation. (1) All nervous impulses are recognized from the outset to be both excitatory and inhibitory.[1] (2) No more intimate connection of the cortex with other parts of the nervous system needs be supposed for combined facilitation and inhibition than for inhibition alone. (3) The brain centres certainly "work over" incoming currents in some fashion, and there is no reason apparently *against* the assumption that their energy may reinforce that of the incoming current, while Exner shows that there are many reasons *for* the supposition. (4) Moreover, in many cases where two organs function together, the limen of their common discharge is lower than the limen of either alone, when the two are separated. In these cases "facilitation" may mean either the lowering of the limen of discharge or the reinforcement of a nerve current. For these reasons among others, the concept of *Bahnung* is valid from the physiological standpoint.

The crucial point in the psychological application of *Bahnung* is found in Exner's assertion that attention increases the intensity of a sensation, but that increase of intensity due to attention is recognized as different from increase of intensity due to the stimulus. Wundt and Külpe also recognize, as indeed every one must, that the intensity of a sensation ordinarily increases in attention, but they ascribe the increase to "associative co-excitation." They deny that attention di-

[1] *E. g.*, Wundt, "*Phys. Psych.*," I, pp. 252, 258.

rectly intensifies a sensation. Exner admits the existence of associative co-excitations, but ranks them as secondary phenomena, and finds them insufficient to account for the facts of attention. When psychology has obtained better experimental control of attention, test-experiments may be devised to decide between the theories of Wundt and Exner. At present, we can make no absolute decision. If we accept the theory of Wundt and Külpe that attention is purely inhibitory, we must explain the increased clearness and efficiency of an idea primarily by the law of the relativity of consciousness, and secondarily by associative co-excitation. At the same time we have no satisfactory schema for the process of inhibition itself.[1] If we accept the theory of Exner that attention is both inhibitory and facilitatory, we explain the increased clearness and efficiency of an idea by correlating them with the facilitatory processes, while at the same time the absence of unrelated ideas is ascribed to coöperative inhibitions; we maintain that attention intensifies sensations, but that intensification due to attention is recognizably different from intensification due to increased stimulus. Both theories must acknowledge that attention does actually reinforce some ideas while it inhibits others. Any instance of close attention in daily life is a picture of the two processes combined. But Exner regards the reinforcement or facilitation as equally fundamental with the inhibition; or rather, throughout his discussion he refers to facilitation as the more fundamental process, preceding or occasioning an inhibition of unrelated regions. Wundt and Külpe reverse the order of the two processes, and, moreover, deny that there is any direct facilitation by central processes; all reinforcement must come by way of associative co-excitation. Neither side has final and completely satisfactory proof for its position; and until we know more of the physiology of the central nervous system, the question whether attention is primarily facilitation or inhibition must be left open.

The conclusions reached by Bianchi, in his recent study of "The

[1] We notice a similar criticism of Külpe's treatment of inhibition is made by Meumann in *L'Année Psychologique*, 1894, p. 514. He says: "I cannot share the opinion of the author that inhibition may be regarded as an independent process. We can think of the inhibition of physical or psychical processes only as the *negative* side of another process. We can only suppose that the energy of one part of the brain is lessened because some other part is expending a surplus of energy. Inhibition, then, does not explain the clearness of a sensation, but just the reverse is true: we interpret the appearance of inhibition in one part of the nervous system as a consequence of the expenditure of too great energy in another part."

Functions of the Frontal Lobes,"[1] must be taken account of in any attempt to explain attention by means of inhibitory or facilitatory processes in the frontal lobes. Bianchi's experiments were performed upon twelve monkeys and six dogs. All the operations were successful except two, and the animals were kept under observation for months or even years. The skill and care with which the whole work was performed cannot be called in question. For his conclusions, Bianchi makes no greater claim than that they give "a plausible explanation of the phenomena observed. . . . . I feel, at any rate, certain of the accuracy of the observations themselves."[2] On the basis of these observations he states very positively that "the frontal lobes are not centres of inhibition. . . . Without attempting to go fully into the theories concerning inhibition and its centres, it will be granted that inhibition depends upon a general psycho-physiological process, and that every part of the nervous system becomes, under different circumstances, either an inhibitory or an inhibited centre. . . . Physiology and psychology swarm with facts showing the error of those who assume a special cortical centre for inhibition."[3]

One statement in this connection we must object to. "We may ask ourselves whether there exists really any centre the function of which is to inhibit, hence whether there exists a faculty of attention. I reply distinctly, No."[4] The "hence" in this sentence is obviously a *non sequitur*.

Bianchi's own "hypothesis is that the frontal lobes are the seat of the coördination and fusion of the incoming and outgoing products of the several sensory and motor areas of the cortex. . . . The frontal lobes would thus sum up into series the products of the sensori-motor regions as well as the emotive states which accompany all the perceptions, the fusion of which constitutes what has been called the *psychical tone* of the individual. . . . With the organ for the physiological fusion which forms the basis of association, disappear also the physical conditions underlying reminiscence, judgment and discrimination, as is well shown in mutilated animals."[5]

Bianchi's general description of the rank and influence of the frontal lobes agrees with that given by Wundt, but their characteristic physiological process is said to be one of coördination and fusion instead of a process of inhibition. This tallies with Exner's definition of *Bahnung*, and it seems at first sight to add much to the probability of Exner's theory. The inhibitory theorists, however, would doubtless maintain that Bianchi misinterprets their concept of inhibition. They could agree with him in regarding the frontal lobes as necessary for the fusion of impressions, and yet explain the relation between the two by assuming that the fusion of certain impressions is possible only when there is inhibition of all other impressions. It must also be remembered that Bianchi's experiments were made upon animals that had already had the use of their frontal lobes for some time. Many of the phenomena that persisted after their removal must have been at some earlier time dependent upon the functioning of the frontal lobes, so that conclusions must be drawn very carefully. Yet, after all due allowance

[1] *Brain*, Part LXXII, 1895, pp. 497-522.
[2] Pp. 519, 522.
[3] Pp. 520, 521.
[4] P. 520.
[5] Pp. 521, 522.

has been made for these two objections to Bianchi's summary disposal of the inhibitory function of the frontal lobes, it still seems to us that his observations tend to overthrow the theory of the purely inhibitory character of these functions. The animals operated upon performed all simple acts in a wholly normal way. With regard to higher functions, they were passive, and unresponsive to stimuli that formerly had excited their higher activities. It is hard to see how such results can be fully explained on the hypothesis that the *sole* function of the frontal lobes is inhibition.

Our restriction of the term "explanatory" to the three types of theory discussed in this chapter leaves us open to the criticism of those who maintain that attention is primarily the direct result of variations in peripheral organs. In answer to the criticism we would say: (1) that the only completed theories of this order are those of Ribot (in so far as he considers attention as essentially motor) and Münsterberg. These have already been reviewed in the preceding chapter; and although each has made positive contributions to our knowledge of the phenomena of attention, it is evident that neither goes far enough to be called an explanation. A third attempt is being made by Dr. Heinrich of Zürich, but at the date of our writing only the first chapter of his work has been published.[1] Moreover, references in an earlier work show that Dr. Heinrich admits the probability of the dependence of attention upon central factors. ("*Die moderne physiologische Psychologie*," pp. 129 and 232.)

CHAPTER IV.

Attention and Distraction: An experimental investigation of the effect of so-called distractions on sensible discrimination.

In the preceding review of the current theories of attention frequent reference has been made to the relation between changes in attention and changes in the intensity of a sensation. At one time attention was frequently taken to be the same thing as an increased intensity.[2] Later it was maintained that attention only *indirectly* heightened the intensity of sensations.[3] Others, again, have regarded the intensification as an essential part of the process of attention, but not the whole process; and, moreover, the intensification due to attention is said to be recognizably different from that due to increase in the intensity of the stimulus.[4] Still a fourth

[1] Ebbinghaus' *Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Bd.* IX, *Heft* 5 *u.* 6, *S.* 342-389.
[2] James Mill, etc.
[3] Wundt, Külpe, etc.
[4] Exner.

opinion holds that attention has an opposite effect, and that "all stimuli appear relatively less when the attention is directed to them from the outset." This view has been advanced by Prof. Münsterberg as the outcome of a series of experiments made in the Harvard laboratory and published in the *Psychological Review*, Vol. I, January, 1894, pp. 39-44. Prof. Münsterberg speaks of his conclusion as "an unexpected result." It seemed to us not only an "unexpected," but also an unexplained result. The published account raised many most important questions, to which it gave no answer. The conclusions drawn by the author could not be accepted on the evidence given. There seemed to be a good opportunity for other interpretations of the numerical results. But no one would be competent to judge between different possible interpretations without repeating the experiments. Since the question was one of great significance for any theory of attention, it clearly demanded further investigation. And it was evident that the first step in our investigation must be a critical examination of Prof. Münsterberg's results, on the basis of a careful analysis of the same and similar experiments. Should our results coincide with his, we should then be in a position to decide independently whether they confirmed his conclusions or seemed to be more reasonably explained through factors not noticed in the original experiments. And if our results should materially differ from those obtained by Prof. Münsterberg, this fact would only emphasize the need of a more careful and complete analysis of the factors included in the experiments.

We may here anticipate our account of our results far enough to state that the greatest value of the experiments seemed to us to lie in their bearing upon a problem not mentioned by Prof. Münsterberg,—the problem of the nature and degree of the distraction employed in order to secure inattention. No one can answer the question of the intensifying effect of attention, on the basis of these experiments, until he has first secured some reliable source of distraction, or some method of measuring the degree of distraction obtained. We think that these particular experiments have no conclusive answer to give to the problem of "the intensifying effect of attention"; but as material for the analysis of so-called "distraction" they have been valuable and suggestive. Accordingly our criticisms and conclusions have been grouped together under the heading of this chapter, "Attention and Distraction."

We shall give (1) a brief account of our own experiments, (2) a comparison of these results with Prof. Münsterberg's, together with a statement of our criticisms and conclusions,

and (3) a discussion of the sources of error discovered, and of the precautions suggested by our study of these errors.

I. *Account of our own experiments.*

In general, Prof. Münsterberg's directions were followed out. The one important exception was a difference in the relative intensities of stimulus used. It seemed to us that the difference judged should be somewhere near the limen of differences; for the object of the experiments was the discovery of a tendency to overestimate or underestimate stimuli under certain conditions, and unless the differences given are comparatively near the limen of difference, the tendency, though it may be present, may give no evidence of its presence in the judgment. We found that in the series with weights, distances and light stimuli, the larger differences used by Prof. Münsterberg were correctly judged by our subjects with such ease and certainty, both when their attention was free and when it was distracted, that these differences were eliminated from the series. The change would inevitably have made any tendency to overestimate or underestimate stimuli more clearly evident in our numerical results than in Prof. Münsterberg's, had any such tendency been present.

Minor variations in the experiments, such as slight changes in the intervals of time or in the manipulation of the apparatus, will be mentioned in connection with the special series in which they occurred. The general directions for the experiments we give in Prof. Münsterberg's words:

"Our problem was to arrange the experiments in such a manner that the intensities of two impressions of moderate strength could be compared, and at the same time the attention be directed toward one and away from the other. In this way we examined intensities produced by light, sound, and the lifting of weights, and also the distances between visible points, the distances serving as measures for the intensity of the sensations produced by the movement of the eyes. The method always employed for diverting the attention was as follows: The subject was directed to give his attention fully to the adding of numbers, which in the case of optical impressions were read to him, and in the case of the auditory impressions were read by him. The adding took place before and during the time the stimulus was present. Since the order of the stimuli to be compared is of great influence upon the judgment, two sorts of experiment were arranged for each series. In one case, the attention was directed to the first stimulus, while the second was perceived with diverted attention; and in the other case, the attention was directed to the second stimulus, while the first was perceived with diverted attention. In order to discover from these series the influence of the attention, independently of other conditions, both series must be compared with the results of experiments in which the attention was either directed to both stimuli, or turned away from both. If we designate attention to the first stimulus by A, and that to the second by A^1, and, correspondingly, the inatten-

tion by I, I^1, we have then for each sense and for the same magnitude experiments with A-A^1, A-I^1, I-A^1, and I-I^1."[1]

We have, then, four sets of results: (1) for visual distances, (2) for light, (3) for weights, and (4) for sounds.

Visual Distances. "The optical distances were given by an apparatus consisting of a black cloth surface, 80 cm. square, upon which were two white points. The vertical distance between these points could be changed by a screw upon the back of the screen and the exact distance moved could be accurately read."[2] The subject opened his eyes at a signal; looked at the points for 3 sec.; at a second signal closed his eyes for 5 sec., while the distance between the points was being changed; at a third signal opened his eyes again, and looked at the points for 3 sec. In every instance a distance of 30 cm. was compared with 28.5, 29, 29.5, 30, 30.5, 31, or 31.5 cm. In half of the experiments it preceded the other member, and in half followed it. It was most frequently compared with 29 and 31 cm., and least frequently with 28.5 and 31.5 cm. The series were not precisely the same for the different subjects in any one of the four groups of experiments, since their ability to discriminate varied considerably; but the same series were used for each subject under all four sets of conditions. We had four subjects and made 400 experiments with each. The following table gives the numerical results:

TABLE I.—*Visual Distances.*[3]

SUBJECT.	A-A^1.				A-I^1.				I-A^1.				I-I^1.			
	Second stimulus correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.
P.	63	29	8	21	69	26	5	21	79	12	9	3	67	20	13	7
W.	66	27	7	20	62	28	10	18	67	24	9	15	73	31	7	16
R.	66	16	17	–1	58	27	15	12	66	17	16	1	76	11	12	–1
M.	66	23	11	12	71	19	10	9	66	24	10	14	68	16	16	0

[1] "The Intensifying Effect of Attention," *Psych. Rev.*, I, pp. 39, 40.
[2] P. 40.
[3] The results in all our tables are given in percentages of the whole number of experiments in each separate series.

We postpone the discussion of these numerical results until we have given the results of the experiments with the other stimuli.

Light. For the light stimuli two rotating disks were used with variable black and white sectors. The time intervals given were the same as in the preceding group. The darkest gray was produced by a black disk with a white sector of 65°, the lightest by a disk with a white sector of 115°. In each instance the subject was given differences of 5°-15° to judge, and care was taken that the number of cases in which the second disk was lighter should equal those in which it was darker. We have results from four subjects, and from 320 experiments given to each subject.

TABLE II.—*Light Stimuli.*

SUBJECT.	*A*-*A*¹.				*A*-*I*¹.				*I*-*A*¹.				*I*-*I*¹.			
	Second stimulus correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.
P.	80	12	8	4	70	11	19	-8	81	9	10	1	75	13	11	2
W.	73	19	8	11	66	11	23	-12	69	17	14	3	75	15	10	5
R.	75	12	13	-1	85	10	15	-5	88	4	8	-4	74	13	13	0
H.	74	13	13	0	80	8	12	-4	78	22	0	22	75	17	8	9

Weights. "The weight was given by lifting a funnel-shaped vessel, held between the thumb and first finger. The elbow rested upon the table, and the weight was raised without movement of the wrist. Weights were put into the funnel in such a way that they could be easily changed."[1] The funnel was lowered into the hand, steadily lifted and lowered, and after 5 sec. lifted and lowered again, but with the weight changed. A weight of 300 gm., including of course the weight of the funnel, was compared with an equal weight and with weights 10, 20 and 30 gms. heavier and lighter. With one subject, *V.*, differences of 50 gms. were used. Five subjects tried the experiments with weights. Each gave 400 judgments, 100 in each series.

[1] P. 41.

TABLE III.—*Weights.*

SUBJECT.	*A-A*¹.				*A-I*¹.				*I-A*¹.				*I-I*¹.			
	Second stimulus correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimat ion.
P.	71	16	13	3	70	25	5	20	78	11	11	0	71	21	8	13
W.	77	16	7	9	64	29	7	22	71	19	10	9	71	20	9	11
R.	69	28	3	25	62	20	18	2	62	37	2	35	71	15	14	1
V.	55	43	4	39	62	34	4	30	64	34	2	32	63	34	3	31
A.	71	25	9	16	63	25	12	13	61	34	5	29	61	27	12	15

Sounds. The sound was made by the striking of an ivory ball upon an ebony plate. "The ball was held by an electromagnet and fell at the breaking of the current. The time between the two sounds was 5 sec. The normal height of the fall, 50 cm., was compared with 35, 40, 45, 50, 55, 60 and 65 cm. A signal preceded the sound, and simultaneously with this the adding of the numbers began as they were read."[1] These experiments were tried by two subjects who had had the practice of all the preceding series. Each subject completed four series of 80 experiments, 320 in all.

TABLE IV.—*Sounds.*

SUBJECT.	*A-A*¹.				*A-I*¹.				*I-A*¹.				*I-I*¹.			
	Second stimulus correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.	Correctly judged.	Overestimated.	Underestimated.	Preponderance of overestimation.
P.	73	15	12	3	78	12.6	9.4	3.2	74.7	13.8	11.5	2.3	72	16	12	4
R.	70	17.5	12.5	5	66	21	13	8	77.5	10.5	12	−1.5	70	18.8	11.2	7.6

[1] *Op. cit.*, p. 41.

The first result to be noticed is that out of the fifteen cases *only two* show the highest percentage of correct judgments in the series without distraction. In six cases the maximal accuracy is reached when the attention was distracted during the first stimulus; and in four cases, the maximum occurs in the series with distraction throughout. How are these results to be explained? Must we conclude that attention is unfavorable to sensible discrimination? The mere figures are of no value in answering this question until they have been interpreted by the introspective evidence gleaned from the subjects during the whole course of the experimentation.

We notice first the evidence bearing upon the nature and effects of the "distraction." The subjects usually found that it acted as a spur rather than as a check to the attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distraction was not continuous, and there came free instants in which the stimulus flashed into consciousness with great clearness and distinctness. Although the subjects were instructed to give their attention to the addition, the fact that they must give some judgment of the stimuli made them instinctively attentive to the stimulus, whenever there was any break in the continuity of the distraction. The series with distraction required greater effort; and if the subject was not in good physical condition, the mind did not pass quickly enough from addition to stimulus and back again. At such times (as also in the infrequent cases when the addition did furnish a continuous distraction) the subjects were completely at a loss how to judge the two stimuli. They seemed to have no memory of them, sometimes even no knowledge of the fact that the second stimulus had been given. The judgments in such cases were nearly as often wrong as right, with a slight tendency to overestimate the unnoticed stimulus.

The important fact, then, with regard to the distraction which appears from the evidence gathered in our experiments, is the fact that in many cases it is *not* a distraction in the required sense. It prevents attention to a stimulus for any length of time, but it by no means prevents a clear and keen perception of an object for a fraction of the allotted interval,—a perception that is evidently as useful in the formation of a judgment as the undisturbed perception during the whole 3 sec. in the series without distraction.

The subjects occasionally referred to a loss of "memory" occasioned by the distraction. A stimulus would be perceived, instantly judged with reference to the preceding, but the judgment would be sometimes forgotten in the course of further distraction. On the other hand, two of the sub-

jects *(R.* and *M.)* found it easier to remember stimuli or judgments, when some distraction was given, than when they had no distraction at all.

We know of no experiments that investigate the relation between the duration of attention and the accuracy of judgment, nor have we any statistics to offer on this matter. But we took several series of experiments with lights and distances, in which the subject merely gave a glance at the second object exposed. The judgments in these series were even a little more accurate than in those where the subjects were allowed 3 sec. for the second stimulus. And those who made the judgments were ordinarily better satisfied with the brief glance than with the long exposure. In fact, after the first few experiments some of them habitually gave the judgment as soon as they had opened their eyes the second time, —not caring at all for a longer observation. They were not cautioned against this mode of judgment, because the experimenter was more concerned with the "goodness," or degree, of their attention than with its duration. Moreover, the sound experiments gave only a momentary stimulus, and yet the subjects did not find that this fact made it any more difficult to judge between the stimuli. The time during which a stimulus is attended to doubtless has a marked effect, however, on the length of time it can be remembered. Thus, in these same experiments the subjects were more inclined to look the full 3 sec. at the first object which they knew they must recall at the end of 5 or 6 sec., than at the second object which could be ignored as soon as the judgment was once made.

To summarize the points that are of importance in the preceding analysis of the distraction used, we would notice: (1) that the distraction is discontinuous; (2) that for the required judgments the *duration* of the attention is of little significance, especially in the last interval of the experiments: the *degree* of attention affects the judgment more than the duration; and (3) that the so-called distraction shortens the duration of the attention, but, as a rule, on account of its lack of continuity, allows the subject brief intervals of attention; and at the same time, by adding definiteness and interest to the task, it heightens the degree of attention. The subject is alert and active; the distraction works, as we have said, more as a spur than as a check. The inevitable conclusion from these observations is a distrust of *any* inferences drawn from such experiments. Until we have some reliable experimental control of attention, until we can objectively regulate its degree, and know the effects of its duration, and the relative value of the various ways in which individuals seek to reinforce attention,—until we have some answers to

these questions, we can have little confidence in the array of figures supposed to represent attention and inattention. We may quote here a passage from Münsterberg's writings:

"Through empty experiments, which have figures as their chief result, psychology can be helped into a dilemma which is as fatal to it as any metaphysical presupposition. Numbers in and of themselves have no value, only concrete numbers can possess any scientific significance; and there is danger of collecting numbers, of examining and explaining these in their relations, without considering the preliminary question of what these numbers really mean and for what psychological process they furnish a measure. If the usefulness of the experimental method is not to be paralyzed by the injuries brought upon it by the ambiguity of indefinite numbers, every investigation must be preceded by a very thorough consideration of what the desired numbers can and must properly signify. And above all, the subject, from whose psychical experience the numbers are obtained, must clothe the numerical skeleton with the flesh and blood of his own most accurate recollection of the conscious processes experienced."[1]

It seems to us that Prof. Münsterberg's condemnation of "experiments which have figures as their chief results" may apply in some measure to this part of his own work. To further justify this conclusion we shall mention here some of the disturbing factors discovered in the course of our work.

(1) In the series with weights we noticed that some subjects relaxed the muscles of the hand and arm after raising the first weight, while others tried to keep them in just the condition assumed while raising the first weight until the second weight was given. This latter method seemed subjectively much more certain and accurate to those who used it than the method of relaxing the hand in the interval, but it was decided that greater constancy of conditions would be secured by the relaxation method, and from the time that the difference was discovered, the subjects let the hand fall loosely during the intervals between the raising of weights.

"The comparative accuracy of the two methods of judging weights was investigated by Prof. C. E. Parrish, of Randolph-Macon College, who has kindly allowed her results to be given here. Series of experiments, from 150 to 225 in number, were given to seven subjects, no one of whom had any knowledge of the question being investigated. Each subject was given weights to raise, and to judge heavier or lighter, in the manner already described in our own experiments. The standard weights were from 250-270 gms., and the differences given were from 10-30 gms. Every series of experiments was given twice. The first time the subject was told to keep the muscles of the hand and arm in precisely the same state they were in while he was raising the first weight until the second weight was lowered into the hand. When the series was repeated, he was told to relax all the muscles of the arm during the interval before the raising of the second weight. The tabulated results are as follows:

[1] Quoted by Heinrich, "*Die Moderne Psychologie*," p. 154.

Discrimination of Weights with Muscles Adjusted and Relaxed.

Subjects.	Number of Experiments.	Percentages of Correct Judgments.	
		Muscles Adjusted.	Muscles Relaxed.
W.	226	63	89
B.	150	77.3	78.6
K.	112	78	92
D.	158	79	86
Br.	184	70	79
H.	166	71	69
M.	176	83	78

From this table it will be seen that five of the seven subjects reached their best results with the hand relaxed. Of the other two, who were both entirely new to laboratory work, the experimenter states that *M.* did not fully relax the hand, and, when the signal was given to have the hand ready for the second weight, tried to put her hand into the same state of tension that it had with the first weight. So that this series really does not fulfill the conditions of the experiment. The variation of the *H.* series is to be explained in a different way. *H.* visualized the weights very definitely, and speaks of "keeping the image in her mind" throughout the interval between the two weights. She saw "a black ring of weight" where the funnel rested on her hand. She kept this image, and compared it with the "ring of weight" seen when the second funnel rested in her hand. When the visualizing element is so strong it is not surprising to find results that are quite different. We are far more accustomed to attending to visual ideas than to the sensations of pressure and contraction accompanying any adjustment of the hand. Trained by daily experience, the attention can be directed much more steadily and successfully to a visual image. And so it happens that in the case of *H.*, where the attention is fixed on a visual idea, a particular adjustment of the hand is more easily and accurately kept than when the attention is directed toward the sensations from the hand and wrist. In the latter case a greater degree of accuracy is secured when the hand is relaxed in the interval, just because the attention is so little used to the investigation and control of muscular and pressure sensations that it fails to preserve a particular adjustment of the muscles concerned. The very effort to attend introduces small changes in the muscular contraction and causes slight fatigue or summation effects, with a consequent tendency to overestimate the second weight.

(2) Another individual variation discovered in the course of the experiments was the difference in the methods of adding numbers. Some visualized the numbers, some depended more upon auditory or tactual-verbal images. As a result

the addition was much less of a distraction to some than to others.

(3) There was an equally marked difference in the way in which the first stimulus was remembered. Some retained visual images of the disks and distances throughout any undisturbed interval, or heard again and felt again the sounds and weights just given. Others let the interval remain empty, so far as possible, and recalled the first stimulus only when the second was given. We believe that these differences are intimately connected with the individual differences in the relative frequency of overestimation in the four series. For the memorial images tend to grow fainter, even as this short time passes, unless a pronounced effort is made to prevent their decrease. The effort, however, is more likely to introduce errors than the tendency it seeks to correct, so that the subjects who retained memorial images made no special attempt to prevent the slight diminution of intensity naturally present. In the series A-A^1 and A-I^1, this factor will have some influence; in the series I-I^1 it could have no part, because none of the subjects retained their images while they were adding numbers. In the series I-A^1, the addition, as a rule, lasted into the intervening period, so that in this series memorial images occurred less frequently. We cannot give statistics from our own experiments to show the relation between the appearance of memorial images and the overestimation of the second stimulus. But after the individual differences with regard to the occurrence of memorial images had been discovered, occasional notes were taken of the subjects and series in which they were most common. The cases noted are those of *P.* and *W.* in the series A-A^1 with disks, and series A-A^1 and A-I^1 with distances; in series A-A^1 and A-I^1 with weights, images were less definitely and less frequently present. Reference to these instances will show that the retention of the image is accompanied by a tendency to overestimate the second stimulus. In the experiments with weights the tendency is weaker in the series A-A^1 than in the series A-I^1. This anomaly, and the occasional appearance of a strong preponderance of overestimation in subjects and series that were quite free from the memorial images, indicate that other factors must take part in producing the tabulated results, and explanations must not depend too much upon the influence of the images in the series where they were specially noticed. Still, on the other hand, their influence must not be ignored. We shall refer to this somewhat later when we take up Prof. Münsterberg's conclusions.

Some light has been thrown upon the last two topics by the results of a series of experiments performed by Mr. F. E. Moyer

He gave three subjects the task of discriminating shades of gray produced by rotating disks under the general conditions of Prof. Münsterberg's experiments and our own. The important differences were as follows: (1) In series I-A^1 and A-I^1, he gave only about 50 experiments, while in A-A^1 and I-I^1 he gave 90 or 100. (2) He gave about twice as many experiments where the only possible error was a judgment of "darker" as where the error must be a judgment of "lighter." Hence the percentages given in his table are not percentages of the whole number of experiments, but only percentages of correct judgments in the number of experiments belonging to each class. (3) Another variation was the restriction of the smaller differences to be discriminated to one class of experiments,—that which allowed errors only in the direction of judging darker. Mr. Moyer's chief object in these experiments was the study of the different ways in which the subjects added and carried the memory of the first disk through the intervals of free and distracted attention.[1] The three differences in experimental conditions just mentioned were introduced for the sake of his own subject. But his tabular results are also valuable for comparison with my own within certain limits not affected by the variations adopted. For since precisely the same series were given by Mr. Moyer to his three subjects, and repeated in all four sets of conditions, the results of the different subjects and sets can be legitimately compared with each other. We also call attention here, as before, to the fact that "overestimation" is used, *in the table*, to mean an error in judging the second disk to be lighter. We do this in order to avoid confusion in the reading and comparison of the different tables in Prof. Münsterberg's article and our own. But, as we have already said, for reasons given under the next topic, it is more often true that the term *ought* to be applied to errors in the direction of judging a disk to be *darker*. Finally, a new set of figures has been introduced into this table to show the whole number of experiments in each class and the number of errors.

With this table before us, let us turn for a moment to the evidence gathered by Mr. Moyer with regard to the ways in which the three subjects combined addition with the discrimination of the different shades of gray.

(1) *Different modes of addition: their value as distractions.* One subject, *R.*, first visualized the numbers given her, in order to get a good, clear memory of them, and then continued the process of addition by auditory images. This subject repeatedly states that she sees the disks clearly *while* she is adding. Both from her own testimony and from her comparative accuracy in many series "with distraction," it is evident that the addition was often no distraction at all. Another subject, *H.*, visualized the first numbers given, but lost the visual image immediately, and added by means of auditory and tactual-verbal images. The disks were not seen during the whole process of addition, but appeared, clearly and distinctly, at certain stages in the addition. After some practice this subject found it much easier to coördinate the addition and the judgment of the disks than at the first. Here, again, the addition ceased to be a serious distraction. The third subject, *P.*, finds that his attention alternates in the same way between the addition and the judgment. His most noticeable peculiarity is the absence of visual and auditory images during addition. He uses tactual-verbal images

[1] Mr. Moyer's own report of this experimental work will be published in a later number of this JOURNAL.

Subject.		A-A^1. Total.	A-A^1. Chances for overestimation.	A-A^1. Chances for underestimation.	A-A^1. Preponderance of overestimation.	I-A^1. Total.	I-A^1. Chances for overestimation.	I-A^1. Chances for underestimation.	I-A^1. Preponderance of overestimation.	A-I^1. Total.	A-I^1. Chances for overestimation.	A-I^1. Chances for underestimation.	A-I^1. Preponderance of overestimation.	I-I^1. Total.	I-I^1. Chances for overestimation.	I-I^1. Chances for underestimation.	I-I^1. Preponderance of overestimation.
R.	No. of experiments,	101	32	69		49	16	33		46	16	36		98	33	65	
	No. of errors,	27	2	25		9	1	8		7	2	5		15	1	14	
	Percentage correct,	73¼	93¾	63¾	−30	81⅗	93¾	75¾	−18	84⅘	87½	83⅓	−4⅙	84.7	97	78.5	−18½
P.	No. of experiments,	104	26	78		44	10	34		56	16	40		94	25	69	
	No. of errors,	28	5	23		12	1	11		14	2	12		38	8	30	
	Percentage correct,	73.7	80.7	70.5	−10.2	72.7	90	67.6	−22.4	75	87.5	70	−17.5	59.6	68	56.5	−11.5
H.	No. of experiments,	95	33	62		60	19	41		52	16	36		90	33	57	
	No. of errors,	24	10	14		13	3	10		18	3	15		27	4	23	
	Percentage correct,	74.7	70	77.4	7.4	78.3	84.4	75.6	−9.8	65	81	58	−23	70	87.8	60	−27.8

throughout, except that for an instant, in the beginning, the figures are sometimes visualized.

The effect of the difference between *R.'s* method, of continuous combination of the two processes, and the method used by *H.* and *P.*, of alternating attention to the two processes, is to be seen in the tabulated results. *R.*'s lowest percentage of accurate judg-

ments is found in the series with no distraction (A-A^1), and her percentage in the series with distraction throughout (I-I^1) is as high as in any of the series. On the other hand, *P.* is least correct in the series I-I^1, and quite even in the percentages of the other three series. (The minimum accuracy for *H.* is in still another series, A-I^1,—a peculiarity that is explained, we think, in connection with the factor considered in the following paragraphs.) Mr. Moyer's conclusions with regard to the topic of this present paragraph may be summed up as follows: (1) The subjects use distinctly different methods of addition. (2) The difference in method results in a marked difference in the degree of distraction occasioned by the addition.

(2) *The effect of images on overestimation.* Mr. Moyer made a record of about fifty cases in which images were carried through the interval, in order to test the inference drawn from my own experiments,—that the tendency of an image to fade away is likely to favor an overestimation of the second stimulus. Just here it was all important to decide what should be called overestimation and what underestimation. It will be noticed that in Prof. Münsterberg's report the records of two subjects were reconciled with the general theory on the supposition that in two cases the stronger stimuli were the *darker* disks, whereas with two other subjects the stronger stimuli were the *lighter* disks. Now the only basis for such a distinction must be the testimony of the subjects that the lighter or darker disks seemed to them to have a more positive character. Varied evidence (most clear with *H.* and least so with *P.*) indicated that for Mr. Moyer's three subjects the darker disks were the more positive stimuli. Hence "overestimation" *should* refer to a judgment of a disk as too dark, although if we tabulated results merely with reference to the physical stimulus rather than with reference to the psychological perception of it, we should define overestimations and underestimations in precisely the opposite way. But Prof. Münsterberg is certainly justified in giving precedence unhesitatingly to the psychological factor here; and we simply emphasize that method of procedure by applying it to all three subjects, instead of confining it to those whose results can not be explained in any other way.

With the interpretation, then, of "overestimation" as a judgment of a stimulus as *darker* than it was, we turn to the following results: In the series with no distraction (A-A^1): (1) when the second stimulus was lighter, *P.* had 36% more of overestimations than of underestimations, *R.* had 2.38% more of overestimations, *H.* had 13.47% more of overestimations; (2) when the second stimulus was darker, *P.* had $16\frac{2}{3}$% less of underestimations than of overestimations, *R.* had no difference, *H.* had 5% more of underestimations (only sixteen experiments).

No data were collected from the other series because images were not noticed in two of them, I-A^1 and I-I^1; while in the third, A-I^1, they were soon interrupted by the giving of the numbers for addition.

The results just quoted for cases in the series A-A^1, in which the memorial image did have an opportunity to exert its full influence, seem at first glance somewhat ambiguous. But the apparent ambiguity has its own significance. We notice that in the first part, when the second stimulus was lighter, *P.* has the largest preponderance of overestimation, *H.* the next largest, while *R.* has only 2.38%. Now *P.* has been proved to be a poor visualizer; *H.* visualizes rather better, but still quite indefinitely; while *R.* is a clear and definite visualizer. Consequently *R.* holds an image well, at-

tending steadily and evenly to its various details; while *H.* and *P.* have a more vague and scanty image, and are likely to find it dying away. The second part of the experiments, those in which the second stimulus was darker, shows precisely the same tendencies, with the exception of those made with *H.;* but this exception is worthless since it is based on only sixteen experiments.

(3) *Comparison of results with Prof. Münsterberg's and our own.* This comparison can be made more intelligibly when we are discussing the bearing of our own results upon Prof. Münsterberg's conclusions. (See p. 58.)

II. *Comparison of our results with Prof. Münsterberg's, with statement of our criticisms and conclusions.*

The first and most fundamental question that arises as one reads Prof. Münsterberg's account of his experiments is a question with regard to the validity of his decision to compare the series A-A^1 only with I-A^1, and the series I-I^1 only with A-I^1. He gives the following reason for thus confining the comparison:

> A-A^1, as well as I-I^1, give the constant error resulting from position, although with a different mean variation. It appears, however, that the results in the two series are different; the overestimating of the second stimulus in the case of A-A^1 being much more marked than in the case of I-I^1. Accordingly, we should compare those series only in which the judgment is made under the same conditions. That is, A-A^1 ought to be compared only with I-A^1, and in the same way I-I^1 only with A-I^1. . . . . Obviously, the actual numbers are valid merely for the relations of these stimuli chosen arbitrarily, and only their relative value, considered as plus or minus, comes in question.[1]

Looking at the results of our own experiments, the first statement which fails to apply to our results is that "the overestimating of the second stimulus is much more marked in the case of A-A^1 than in I-I^1." We found several cases in which this was true, but there were also several cases in which it was not at all true. Two subjects in the series with weights, two in the series with disks, two in the series with sounds, and one in the series with distances, overestimate the second stimulus more frequently in the series I-I^1 than in the series A-A^1, while two more show a preponderance in the other direction of only 1 and 2%. So that nine out of the fifteen cases fail to confirm Prof. Münsterberg's comparison.

It might still be maintained, however, that although there is no constant difference, in one direction or the other, between A-A^1 and I-I^1, the reason given (independently of the relative overestimation) for limiting the comparison holds good, viz., that "we should compare those series only in which the judgment is made under the same conditions." This would be a sufficient reason if the conditions under which the judgment

[1] *Op. cit.*, p. 40.

is *finally* made were the only conditions affecting the overestimation or underestimation of the second stimulus. But, in the first place, we have already referred to one factor (*i. e.*, the presence of memorial images) that in certain subjects exercises considerable influence in the series A-A^1 and A-I^1. And, in the second place, we maintain that the judgment must be affected quite as much by any circumstance that exerts an influence on the memory of the first stimulus as by the conditions existing at the moment a judgment is made. Since it is quite as probable that the memory of the first interval is affected by the way in which the intervening period is passed as that the final judgment is affected by the conditions under which the second stimulus is received, we maintain that *all four* series must be compared in order to make due allowance for both sorts of influence. Indeed, if either one is to be ignored, we think it should be just the one that Prof. Münsterberg has considered all important. For what *is* this "difference in the conditions under which the judgment is made" except the very matter under investigation, *i. e.*, the difference between free and distracted attention? And it seems strange that on this basis two series should be cut off from comparison with the very two that exhibit the other phase of the problem in question.

If, however, setting aside Prof. Münsterberg's restriction, we do compare all four series, then, in order to sustain his conclusion that "all stimuli appear relatively less when the attention is from the outset directed to them," the series A-I^1 should show a relatively greater, and the series I-A^1 a relatively smaller, preponderance of overestimation of the second stimulus than A-A^1 or I-I^1. Examining Prof. Münsterberg's results, we find this to be true in only three cases out of ten. In our own results it holds good in only five cases out of fifteen. We take this as further evidence that these series are not what they profess to be; they are not pure experiments with free and distracted attention.

On the other hand, even if we should accept the restriction to which we object, and follow Prof. Münsterberg's example in comparing series A-A^1 only with I-A^1, and I-I^1 only with A-I^1, our results still fail to harmonize with his. In thirty comparisons sixteen agree and fourteen disagree with his statement. So that whether we accept or reject the restriction, we are wholly unable to accept the conclusion that stimuli appear relatively less when attended to.

Reference to the tabulated results of Mr. Moyer's experiments given on page 55 will show that his results work against Prof. Münsterberg's conclusions in the same way that my own do. As they were performed wholly independently and for the investiga-

tion of a different problem, the concurrence of the two results is worthy of consideration. Mr. Moyer's experiments, like my own, fail to confirm the conclusion that attention lessens the intensity of a stimulus. If we compare his series in accordance with Prof. Münsterberg's directions, we find that out of six comparisons three agree and three disagree with Prof. Münsterberg's conclusion.

Our second criticism of Prof. Münsterberg's experimental work is a criticism of the way in which he has handled his numerical results. Every error in judgment is given the same numerical value, whether the stimuli wrongly judged were equal, nearly equal, or widely different. Such an evaluation of results is quite as capable of concealing a tendency as of revealing it. We have of necessity followed the same method since our first object was a comparison of results. At the same time we think it must be condemned as untrustworthy and unscientific. Only the discovery of sources of error which convince us of the worthlessness of the experiments in connection with their original problem, allows us to leave them without recasting the results on a more scientific basis of evaluation.

In the third place, if we turn to Prof. Münsterberg's *explanation* of the conclusions drawn from the experiments, we find an explanation that will not apply to attention in general or to the kind of attention used in our own work. The explanation is valid only where there is a pre-adjustment of the attention to some particular stimulus.

To draw out our meaning further, we quote a few sentences from the last paragraph of the article:

"The explanation seems to me to rest in the fact that we must always judge intensities relatively, the standard being in our muscular tensions. . . . . Let these be previously strengthened by expectant attention and the stimulus will appear weaker than if the stimulus itself were to arouse reflexly all the corresponding muscular tensions. . . . The feeling of intensity comes to represent the intensity of a stimulus only through its relation to the subjective sense of strain. If we purposely strengthen the subjective strain simultaneously with the strains aroused by the stimulus, the stimulus will indeed appear stronger, because we interpret the tension as the result of the stimulus. . . . If, on the other hand, the tension precede the stimulus as an element in the preparatory adjustment of the attention, and if consequently it be interpreted by consciousness, from the outset, as a subjective function, the increase of the tension aroused by the stimulus can appear only slight, the ratio of the two intensities has become reversed, and accordingly the stimulus is slightly underestimated."

All this may be quite true. Such a possibility of weakening an impression by a certain preparatory adjustment of the attention was noticed by G. E. Müller nearly twenty years ago, and it has been verified many times. But it is only true of attention when it is working under peculiar conditions and

in one particular way. Prof. Stanley has emphasized this point in his discussion of Prof. Münsterberg's article. After some reference to the genetic development of attention, he says:

"Believing, then, that sensation intensities are bound up with attention intensities as a general fact of mind, we were interested to see how Dr. Münsterberg's experiments would bear on this law, to which he alludes in his opening remarks as the scope of his inquiry. However, we discover that it is only a certain kind of attention, expectant, and a certain kind of this, too much expectancy, that is really treated, with the result that sensations of light, sound, etc., are rendered less intense when we set our attention at too high a notch."[1]

In our own experiments there was no preparatory adjustment of the attention to a definite intensity of stimulus,—no "expectant" attention to any specific memory image. Attention in our case meant, to use Exner's terms, an inhibition of stimuli in general, and a readiness for, or a "facilitation" of, a certain class of stimuli. It cleared the field of all other impressions and said, "Something is coming *this* way. Ready, look!"

In the passage just quoted Prof. Münsterberg maintains that the subjective strain due to a preparatory adjustment of attention is distinguished in consciousness from the strain aroused by the stimulus, but if the subjective strain is strengthened simultaneously with the strain aroused by the stimulus, the two are not distinguished; "we interpret the tension as the result of the stimulus." This brings forward once more the question discussed in the preceding chapter,—the question of the distinction between increase of intensity due to attention and to changes in the stimulus (pp. 37-38, 40-41). We think that the view there cited from Exner is the only one that is consistent with the facts of experience. For instance, when we are listening to the far-away striking of a clock, we do not confuse the subjective tension with the tension aroused by the stimulus. Although the distinction of the two is more difficult when they occur simultaneously, it cannot be impossible; for the same data are given in consciousness when their occurrence is simultaneous as when successive, only they are more obscure in the former case than in the latter. The quick notice and accurate memory of an expert, however, will render possible the analysis of either complex state. Indeed, unless there were certain *measures* of the facilitation due to attention by which it could be distinguished from the intensity of the stimulus, attention would obscure and interfere with every case of sensible discrimina-

[1] "Attention as Intensifying Sensation," H. M. Stanley, *Psych. Rev.*, II, p. 55.

tion,—and we believe that no one holds this to be true. On the other hand, we do not wish to dispute Prof. Münsterberg's statement that expectation of too great or too small a stimulus causes underestimation or overestimation of the stimulus actually present. But we affirm that such expectation is not an essential element in attention. We are more and more convinced that there is no necessity that attention should produce either result; and we hope to show in the next section that both effects may more properly be traced to certain sources of error and of individual variation noticed in the course of our experiments.

III. *Discussion of the sources of error observed and the precautions suggested.* The mention of the sources of confusion discovered in our own experiments may be the best conclusion of our present account. Our other results have been largely critical and negative. We may hope that their worth is not decreased on this account, since the whole subject of attention is still in so chaotic a state. Negative criticism of conclusions too hastily reached is specially needed in the first stage of experimentation in any subject,—and experiments upon attention, with the exception of those upon oscillations of the attention, have hardly passed beyond the first stage. Moreover, if negative criticism can indicate clearly enough the sources of error in the works which it criticises, it leads in the end to a knowledge of how the errors are to be avoided, and shows, in part, at least, the correct method for the solution of the problem.

We conclude, from our own experiments, that the most probable sources of error are as follows: (1) *The pre-adjustment of the attention.* There should be objective aids to secure, so far as possible, constancy in the manner and degree of the adjustment. Any means for securing similarity of conditions at the outset will do much toward ensuring trustworthy results. We have said that in our own experiments there was no definite pre-adjustment,—no preparatory recall of a particular stimulus. Prof. Münsterberg's closing paragraphs indicate that he did have such preparatory adjustments in his experiments. To avoid these differences and to prevent the appearance of other disturbing factors in the first stage of the experiment, it would be well to start with the same stimulus in all the experiments. Let the subject know that the first stimulus is always the same, and let him keep account of its apparent variations, if there are any. Such a partial use of the "method with knowledge" would be of value in giving the attention a definite direction. It would help to insure the same degree as well as the same kind of attention or of preparatory adjustment. This single

precaution would go far, we believe, toward correcting or eliminating the fluctuations, contrast effects, variations in adjustment, etc., that are otherwise likely to occur during the exposure of the first stimulus and during the interval between the two stimuli.

(2) The experimenter must use some measure, however crude, of the *continuity of the distraction.* For this purpose he must take notice (a) of the regularity or irregularity of the rate at which the subject works; (b) of the most effective kinds of distraction, and (c) of the ways in which the subject treats whatever is assigned for a distraction. Under these three topics we have a few suggestions to offer as the fruit of our experimental work.

(a) It might be advisable to arrange the distraction and the apparatus so that the rate at which the subject does whatever is given him to do, may be recorded. This time record would reveal clearly—and often at the moment, and to the subject himself—the irregularity and discontinuity of his attention to the objects discriminated or to the distracting stimulus. This information would *tend* to make him more even and uninterrupted in his attention to one or the other. The record would also tell the experimenter something of the degree and value of the distraction. Yet too much must not be expected from this device. (b) Our experiments, and others conducted in the same laboratory, indicate that in spite of every effort exerted to keep the distraction continuous, any task of so intellectual a nature as addition will permit intervals of keen attention to other objects. It was noticed, however, that when the addition was performed while there was any strong affective coloring of the subject's consciousness, it was usually a successful means of distraction. Affective reactions rarely, and only accidentally, accompany the addition of numbers, the recognition of liminal stimuli, the perception of regular intervals of time by metronome beats, and the other similar distractions which have been used in the experimental study of attention. But it is possible that other stimuli may have a much stronger affective tone. This is in general, true of all strong odors.[1] And beside these, each individual has certain classes of perceptions which are very likely to arouse strong feeling. Singing aloud, reciting with careful expression one's favorite poems, reading or writing backward, recalling details of a beautiful landscape or painting,—these and many other such tasks will affect different individuals very differently, and for certain subjects we have found them to be quite a satis-

[1] This fact was evident in the experiments by Mr. Moyer already referred to, p. 54.

factory distraction. (c) Whatever distraction may be used, it is evident that the experimenter *must* know, as well as he can, *how* the different subjects are treating the task assigned for distraction. It is as important to know this as to know the way in which the subjects attend to the stimuli to be discriminated. It would seem to us quite unnecessary to emphasize directions so apparent as these if they had not been unpardonably ignored in the experimental study of attention. As we have already said, Mr. Moyer's investigation of addition as a means of distraction, showed that it was treated differently by each of the three subjects. One was visual and auditory-verbal; one was auditory-verbal and tactual-verbal; the third was quite purely tactual-verbal in his method of adding. And these differences affected the results. The experimenter cannot give any reliable interpretation whatever of his results without an analysis of these complicating factors. Control of the variations in the treatment of the distraction may be impossible, but this fact will not destroy the value of the experiments if only the variations are known and if their influence is carefully traced.

(3) The subject's *methods of remembering* a stimulus must be noticed in the same way that his ways of attending or of pre-adjusting the attention are to be observed. The most important instance under this head that we can give from our own experiments is the instance of the presence and absence of memorial (reproductive) images in different series. We have only to refer again to the account given of Mr. Moyer's experiments to show the influence of the memorial image upon the overestimation of the second stimulus. Besides the persistence of definite images, other mnemonic aids may be used and may affect the results. Certain subjects, who do not steadily retain any images, recall them at stated intervals, usually with each inspiration or expiration of the breath. Under these circumstances the coincidence of the second stimulus with the moment for the recall of the image will make considerable difference in the accuracy and the direction of the results. Any other aids to memory—naming, localizing, etc.—may be used by the subjects; and here again it is necessary for the experimenter to know what factors are entering into the experiments.

(4) Results obtained by one method, *e. g.*, attention in sensible discrimination, must be *tested by experiments in other methods*, *e. g.*, reaction to a stimulus. This latter method has been well developed by Patrizi.[1] By means of an

[1] "*La Graphique Psychométrique de l'Attention*," *Archives Italiennes de Biologie*, Vol XXII, pp. 189-196.

electrically driven Baltzar kymograph and an electric circuit, including a Deprèz signal and an electro-magnetic hammer, he secured a record of sixty successive reactions made within an interval of two minutes. The stimulus recurred regularly at intervals of two seconds. The subject reacted to the stimulus as soon as possible, and the record of stimulus and reaction left on the rotating cylinder showed how much time had intervened between the two. The hammer could be replaced by a Geissler tube or a simple shock apparatus when it was desirable to use other kinds of stimulation. By means of this apparatus, Patrizi obtained a graphic record of the degree or goodness of the attention, since (other things equal) it is evidently inversely proportional to the time of the reaction. The method has also the merit of investigating attention under purer conditions than are found in other methods, because the quick recurrence of the stimulus helps very much to prevent the wandering and "unpreparedness" of the attention. And the fact that "the stimuli given are always well above the limen of any sense department, eliminates the effects of the adjustment and muscular fatigue of the peripheral sense organs that have complicated all the experiments on oscillations of attention." Experiments performed by Patrizi's method could be advantageously compared with those by another method in two ways: (1) It can be used to find the comparative value of different distractions; and (2) after the value of the distraction is known, the relations of different degrees of attention and inattention shown in the numerical results of one method can be verified or modified by the graphic results of this other method.

In closing we wish to give brief notice of "An Investigation of the Distraction of Attention" made by Bertels.[1] It is the only thesis on the subject that we have yet come across, and deserves consideration on that account. But the author's work does not touch the problems with which we have been dealing. Bertels started with three questions in mind. "1. How is distraction affected by the intensity of the distracting stimulus? 2. How is it affected by the interval between the test-stimulus and the distracting stimulus? 3. How is it affected by the quality of the distracting and the test-stimuli?"[2] But he had time to investigate only the second question—the effect of the interval between the two

[1] "*Versuche über die Ablenkung der Aufmerksamkeit.*" Dorpat, 1889. A. Bertels.

[2] *Op. cit.*, p. 10.

stimuli. Visual stimuli were used in all the experiments. The distracting stimulus, as well as the test-stimulus, was given only for a moment. Light was admitted for an instant into a tube leading to one eye, and then after an interval, light was admitted to another tube containing two Nicol prisms. The distracting effect of the first stimulus was measured by the angle through which one of the Nicols had to be turned before the second light could be discerned. The most important result of his experiments was the discovery that the first stimulus acted more as a signal-stimulus than as a means of distraction. It distinctly aided the attention, most of all at an interval of $2\frac{3}{5}$ seconds, and at intervals that were multiples of this number. So that the thesis tells us nothing with regard to distraction, except that what was expected to act as a distraction acted otherwise.

Summary of the four chapters.

The introductory chapter briefly discussed certain physiological concepts, which play an important part in theories of attention; *i. e.*, the question of the nature and extent of cerebral localization, and the question of neural reinforcement or facilitation (*Bahnung*), and neural inhibition (*Hemmung*).

The second chapter analyzed the "descriptive" theories of attention, or those which deal chiefly with the effects and secondary phenomena of attention. These theories describe attention as merely motor (Ribot and Münsterberg), merely sensory (Bastian, Marillier, etc.), or as sensori-motor (Waller). The discussion of these three classes involved reference to the controversy over sensory and motor centres in the cortex, the correlation of consciousness with efferent nerve currents, and allied topics.

In the third chapter the "explanatory" theories of attention were presented—those which seek for the fundamental principles in the process. The three types found here regard attention as a facilitation (G. E. Müller), as an inhibition (Wundt and Külpe), or as a combined facilitation and inhibition of stimuli (Exner). After a review of each theory separately, we discussed the fundamental differences between them, and especially the problems still left unsettled by one or all of these theories. Some of the most important topics were the relation between attention and the increased clearness of an idea, the "feeling of activity," inhibition, associative co-excitation, reinforcement or facilitation, changes in intensity due to attention and due to variations in the stimulus.

The fourth and last chapter, under the title, "Attention and Distraction," gives an account of an experimental investigation of the effect of so-called distractions on sensible discrimination. The experiments are, in part, a repetition of those made by Prof. Münsterberg in his study of "Attention as Intensifying Sensation." The negative and critical results of our work show that the experiments do not sustain Prof. Münsterberg's conclusion that "all stimuli appear relatively less when attention is directed to them from the outset." (1) The distraction acted more often as an incentive or spur than as a check to the attention, and this fact renders the experiments worthless for their original purpose; (2) Disturbing factors were noticed which materially affected the numerical results. These factors were not considered, and no precautions were taken against them in the original experiments. Among the most important we would mention the effects of the presence or absence of distinct memory images, different ways of treating the distraction, and different methods of judging weights. (3) The original experiments treat only a certain kind of attention, expectant, and a certain degree of this, too much expectancy; their results, even if they were wholly accurate, could not apply to attention under any other conditions. (4) An unscientific treatment of the numerical results destroys the force of any psychological conclusions based upon them. The positive results of our work have taken the form of certain directions advisable for further experimentation. They refer: (1) to a pre-adjustment of the attention; (2) to the continuity of the distraction; (3) to methods for remembering stimuli; (4) to the testing of results obtained from one method by those secured from other methods. The chapter closes with brief mention of an earlier study of distraction by Bertels.